

# Essays on Urban Labour and Housing Markets

Michael Aaron Amior

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# Declaration

I, Michael Aaron Amior, confirm that the work presented in this thesis is my own. Where information has been derived from other sources, I confirm that this has been indicated in the thesis.

Michael Amior

# Abstract

This thesis examines the operation of urban labour and housing markets. I bring new insights to old questions about migration, unemployment and homeownership.

The first essay studies the impact of immigration on the wages of native-born workers. In standard competitive models, the effect comes entirely through changes in marginal products of different labour types. But, I argue that firms with monopsonistic power can exploit the lower reservation wages of recent migrants by cutting wages for natives and migrants alike. I present evidence from cross-city variation in local skill distributions, wages, and employment rates.

The second essay looks at why higher skilled workers are more likely to migrate long distances within a country. It is commonly argued that they face comparatively low migration costs. But, US survey evidence on reported reasons for moving suggests this explanation is at best incomplete. I argue that high skilled workers are relatively mobile, more fundamentally, because of larger potential gains from a successful job match.

The third essay documents descriptive facts on regional unemployment differentials. In the UK, unemployment has remained persistently high in less productive cities since the 1980s. But, there is no such relationship in the US: local populations adjust quickly to meet local demand. I speculate that relatively generous out-of-work benefits in the UK may allow unemployed workers to remain in poor-performing cities, while low local housing costs discourage them from searching elsewhere.

The final (co-authored) essay focuses on the determinants of homeownership. It is commonly argued that households bring forward their home purchase because of uncertainty over future house price fluctuations. But, using a life cycle model, we argue that households are more likely to respond to price risk by increasing their liquid savings. We present supporting evidence from cross-city variation in ownership rates and loan-to-value ratios.

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# Chapter 1

## Introduction

Interest in regional issues has grown in recent years, as local disparities in economic outcomes have become more visible following the Great Recession. In this thesis, I address a range of questions pertaining to urban labour and housing markets, drawing from evidence from the US and UK. A critical force shaping the evolution of many cities is immigration, and Chapter 2 revisits the old question of how it has affected the wages of native-born workers. The subject of the next chapter is the geographical integration of labour markets. I discuss why higher skilled workers tend to migrate more between cities. The subject of Chapter 4 is the persistence of high levels of unemployment and economic inactivity in some British cities. Another important distinction between cities is the extent of homeownership, and Chapter 5 (co-authored with Jonathan Halket) exploits this geographical variation to study the impact of price risk on ownership decisions.

Each of these chapters are intended to be free-standing, but it is useful to provide a brief summary of their content and - in the case of the essays on labour markets - to draw out the parallels between them. An important theme in the initial chapters is the role of search frictions and imperfect competition in labour markets. Great progress has been made in this field in recent decades, building on seminal work by Diamond (1982), Mortensen (1982) and Pissarides (1985), who jointly received the Nobel Prize in 2010. One goal of this thesis is to bring new insights from this literature to areas of labour economics traditionally dominated by the competitive paradigm.

In Chapter 2, I argue that competitive models may significantly understate the labour market impact of immigration. Despite the great energy devoted to this question, the literature is still devoid of consensus, with many studies (e.g. Ottaviano and Peri, 2012) arguing the wage effect is small. I take a new approach by studying wage effects in an imperfectly competitive world. In this context, workers' wage demands play an important role in wage determination. The evidence suggests new migrants have lower reservation wages than otherwise identical native-born workers (e.g. Chiswick, 1978; Nanos and Schluter, 2013). This may be due to higher job search costs, ineligibility for social transfers, risks associated with unauthorised status or heavier discounting (since many migrants intend to work in the host country for a limited period). As a result, in cities experiencing sustained inflows of new migrants, employers with market power can exploit these lower wage demands by cutting

wages for natives and migrants alike. This is consistent with public perceptions of migrants “undercutting” long-standing wage norms in the affected markets.

I demonstrate how this wage effect arises in an equilibrium model with wage-posting firms. This exercise builds on work by Albrecht and Axell (1984), who study wage and employment outcomes where workers have heterogeneous leisure values. Following Albrecht and Axell, the monopsonistic power of firms originates from random matching between workers and firms. For simplicity, I assume that firms cannot discriminate between natives and migrants in wage-setting. But, the broad result will hold as long as the markets for native and migrant labour are not entirely segregated.

It is difficult to identify this effect of reservation wages in the data, but I provide some indirect empirical evidence from cross-city variation in the US. Specifically, in recent decades, the wages and employment rates of natives in low skilled occupations (where migrants are concentrated) have fallen in popular migrant destination states relative to elsewhere. A competitive model can explain this if the relative supply of skilled labour has fallen in these states; but this has not happened because of large geographical displacement of lower skilled natives. I also show that these patterns cannot be explained by selection of workers across states, and I propose lower migrant reservation wages as an alternative explanation.

In Chapter 3, I show how frictional models can also shed light on how labour markets operate geographically. It is well known that higher skilled workers are more likely to migrate long distances within a country, but the reasons for this are still under dispute. Evidence has accumulated that low skilled workers are less likely to leave cities suffering declines in employment. Given this limited supply response, it is often argued that the low skilled face significant migration costs, whether due to credit constraints, lack of information or home attachment. This debate has gained more prominence since the Great Recession, as policymakers try to address pockets of low skilled unemployment in blighted areas. Moretti (2012) has proposed that the US federal government fund relocation grants to help workers in this predicament.

This focus on migration costs is natural, given the pervasiveness of the competitive urban framework proposed by Roback (1982). There, migration is treated as a form of spatial arbitrage between distinct local labour markets, with workers comparing local wages, amenity values and housing costs. Any sluggishness in this arbitrage process in low skilled markets must then be explained by large unspecified moving costs. But, in this chapter, I consider an alternative world where workers search for work in multiple cities simultaneously. And I argue the obstacles to low skilled mobility are precisely those “frictions” which explain the coexistence of unemployment and vacancies more generally. These frictions are larger in low skilled markets, because smaller job surpluses discourage search effort (on both sides of the market) and job creation. And, in turn, these paltry surpluses have their source in low average productivity and low dispersion in match quality.

I motivate this perspective with new evidence from the Current Population Survey (CPS) in the US. Since 1999, the CPS has asked workers who changed residence for their primary reason for moving. It turns out that, in any given year, better educated workers are much more likely to move county or state for the sake of a specific job match; and the same is

true of workers in higher skilled jobs *within* occupation groups. But, they are also *less* likely to move for the sake of family, housing or amenity reasons, or to look for work. This is a natural consequence of a large job surplus, which is more resilient in the face of external shocks. I support these claims with evidence from other sources on skill differences in earnings processes, market tightness and search behaviour.

Having said that, it is important not to exaggerate the contribution of geographical immobility to unemployment. Seminal work by Blanchard and Katz (1992), based on time series evidence, finds that local labour supply is quick to respond to local demand shocks in the US. And, Sahin et al. (2012) show that geographical mismatch between unemployed workers and job vacancies did not contribute to the rise in unemployment in the Great Recession.

In a similar vein, I document in Chapter 4 that working-age individuals are no more likely to be unemployed or economically inactive in less productive cities (identified with lower average wages) in the US, once I control for observable characteristics. Instead, these cities tend to be significantly smaller in size - which is consistent with an elastic supply of local labour. A notorious example is Detroit, whose population more than halved since a peak of 2m in 1950 and where 23% of homes are unoccupied<sup>1</sup>.

However, a footloose workforce is not a standard characteristic of advanced economies. In the UK, since the early 1980s, I show that less productive cities have been characterised by significantly higher rates of joblessness. Unemployment rates in 2010 ranged from 5% in the most productive cities to 10% in the least (with the variation driven by younger workers), and economic inactivity among 16-64s from 20% in the most productive to 25% in the least (driven by earlier retirement). The observable characteristics of local workers can only explain a third of this variation. Furthermore, the cross-city relationship between wages and population is much weaker than in the US; this also is consistent with weak migratory responses to shocks.<sup>2</sup>

Out-of-work benefits are significantly more generous in the UK than the US, and this could help explain why unemployed and inactive workers can survive long-term in less productive cities. But, the mere ability to survive is not sufficient reason to stay put, and welfare generosity is an inadequate explanation for why regional shocks from the 1980s have persisted for so long. Instead, I speculate that local housing markets have played an important role. Estimates from Gibbons et al. (2011) suggest that local spending power, accounting for cross-city variation in housing costs, is in fact significantly *higher* in less productive cities. As such, the prospect of a better quality of life in work may discourage local unemployed workers from searching for jobs elsewhere.

One possible theory is that, following the industrial shocks of the 1980s, housing costs in the affected cities fell more than wages because of the durability of local housing stocks. While human capital was destroyed (as skills specific to the old industries were rendered valueless), the local housing stock was largely preserved. This resulted in an over-supply

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<sup>1</sup>Census Bureau, 2010

<sup>2</sup>London is a unique and important outlier, with both high wages *and* high unemployment; I suggest that significant and persistent immigration to the city might help explain its distinctiveness.

of housing relative to labour, so housing costs fell relative to wages. To sustain such an outcome, there must be some downward stickiness in local wages - otherwise, wages would fall to clear local unemployment. This might be driven by benefit or wage policies (such as the national minimum wage or national public sector pay scales) which bind more in low-wage areas. Also, early retirement from the labour force is likely to reduce the pressure on wages through a hysteresis effect (Blanchard and Summers, 1986). The key point is that the benefit system, together with the role of housing markets, might help explain why local unemployment is more persistent in the UK than US - without resorting to the popular perception that Americans are intrinsically more geographically mobile than Europeans.

The fact that these patterns in the UK pertain largely to low skilled workers is consistent with the story in Chapter 3. Specifically, better educated workers pay less attention to regional differences in *average* wages and *average* housing costs, because they are more concerned about finding the best possible job match. As a result, they search more broadly geographically for jobs, so local pockets of unemployment do not materialise.

Chapter 5, based on joint work with Jonathan Halket, moves away from labour markets and focuses on the determinants of homeownership. Housing has traditionally been analysed by economists as a financial asset with relatively risky returns. But, housing also serves an essential function in its own right: everybody needs a place to live. In recent years, economists have argued that ownership is a form of insurance against rental price volatility (Sinai and Souleles, 2005). Or, if households intend to own at some point in the future, they may insure themselves against house price risk by bringing forward their ownership decision to “get on the housing ladder” (Banks et al., 2010).

Using a calibrated life-cycle model, we argue that ownership is too blunt an instrument for many households interested in insurance. Instead, households are more likely to respond to price risk by accumulating liquid savings. We present supporting evidence from variation across US cities. Riskier cities tend to have significantly lower ownership rates (conditional on household income and other observed characteristics) and lower loan-to-value ratios at purchase. Our model suggests the lower ownership rates reflect the fact that housing tends to be more costly in riskier cities. The loan-to-value patterns are driven by geographical variation in price risk: since households save more in riskier cities, they put down comparatively larger deposits when they eventually purchase.

# Chapter 2

## “Cheap Labour”: Immigration and Wages Revisited

### 2.1 Introduction

#### Migration in non-competitive markets

It is commonly accepted that, if capital is perfectly elastic, native workers benefit on average from immigration. This result follows from a competitive model with constant returns to scale. If natives and migrants are not perfect substitutes in production, migrants will be paid less than their contribution to output; and assuming zero profits, this “immigration surplus” will be returned to natives (see Borjas, 1995).

In this study, I show that the immigration surplus argument may fail if workers are not paid their marginal product. In competitive models, any contraction of wages reflects a decline in productivity. However, under imperfect competition, wage effects can materialise *even* with productivity held constant. Specifically, if migrants are willing to accept lower wages than natives, a rising share of migrants (with productivity unchanged) will encourage monopsonistic employers to cut wages for natives and migrants alike.<sup>1</sup> This story of “cheap” migrant labour undercutting native wages has strong resonance in the public consciousness<sup>2</sup>, but has largely been neglected by economists.

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<sup>1</sup>This idea is closely related to Beaudry et al. (2012), and the comparison is instructive. They show, using US data, that the wage bargain in a given job is responsive to local industrial composition (keeping productivity fixed): in a bargaining model, workers in cities dominated by high-paying industries have more attractive outside job options. In both this study and theirs, wages respond to changes in workers’ reservation values. Here, this change originates from the composition of *workers*. But there, it comes from the composition of *industries*. I am grateful to David Green for this observation.

<sup>2</sup>This story is popular across the political spectrum. On the left, union leader Terence O’Sullivan claims “workers don’t depress wages. Unscrupulous employers do” (New York Times, 2009). Ed Miliband, the current leader of the Labour opposition in the UK, believes that “employers should not be allowed to exploit migrant labour in order to undercut wages” (Miliband, 2010). And on the right, Hanson (2013) writes in the National Review that “the bargaining power of other minorities, Latino- and African-American citizens especially, is undercut by illegal labour.”

Why should the mechanism behind the wage effect matter? There are two reasons. First, several studies have estimated wage effects based on calibrations of these models, rather than relying purely on the data for identification. Recent work using this method (Card, 2009b; Manacorda et al., 2012; Ottaviano and Peri, 2012), allowing for imperfect substitution between native and migrant labour, has tended to find only a weak impact on low skilled wages. But, Borjas et al. (1997) note that such calibrations may underestimate the wage effects if there are price changes which are not entirely the outcome of quantity changes. Second, there are important policy implications: in contrast to a competitive framework, the frictional model implies that more effective assimilation of migrants into the labour market can support the wages of natives.

The idea that recent migrants have lower reservation wages than native-born workers is well-rooted in the literature.<sup>3</sup> Much of this discussion originates from early studies of migrant assimilation in the labour market. Chiswick (1978) suggests that recent arrivals are willing to accept low wages at the beginning of their stay, but their wage demands grow as they gather more information about job opportunities in the host country. Using Portuguese employer-employee matched data, De Matos (2011) finds that one third of this wage catch-up is due to firm heterogeneity - as migrants move to larger, better-paying firms within the same occupations. Nanos and Schluter (2013) use a structural on-the-job search model with wage-posting to purge the native-migrant wage gap of productivity differences. They find that reservation wages are generally lower for migrants, but also more dispersed. The gap is largest for low skilled service workers, but small in skilled blue-collar occupations. Dustmann et al. (2013) link the idea that migrants are paid below their marginal product with skill “downgrading”: they tend to work in jobs that are lower skilled than their measured education would suggest (see also Eckstein and Weiss, 2004).

However, there are few (and only very recent) examples in the economic literature which assess the impact of low migrant reservation wages on native outcomes. Chassamboulli and Palivos (2014) and Chassamboulli and Peri (2014) outline theoretical models where migrants’ low wage demands actually *benefit* natives. This seemingly counterintuitive result arises from the assumption of random matching combined with Nash bargaining. Specifically, firms respond to cheap migrant labour by creating many vacancies. And as markets become tighter, natives benefit from higher job finding rates and more favourable ex post wage bargains. Of course, this intuition collapses in a wage posting model, where firms set wages ex ante. And importantly, there is evidence that wage posting is more common than bargaining in low skilled markets (Hall and Krueger, 2012).

On the empirical side, Malchow-Møller et al. (2012) address these questions using Danish employer-employee matched data. They find that, within firms, immigrant employees put downward pressure on native wages; and they argue that this is because immigrants have lower reservation wages. Also, in research conducted concurrently with this study using French data, Edo (2013) finds that non-naturalised migrants put downward pressure on

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<sup>3</sup>Note that reservation values may manifest themselves in workplace amenities as well as wages. Orrenius and Zavodny (2009) find that migrants, and especially those with poor English skills, tend to work in riskier industries and occupations (with higher fatality and injury rates); this finding contradicts earlier results from Hamermesh (1998).

native employment rates; but naturalised migrants have no effect. These results exploit variation in changing migrant shares across age-education cells.

In contrast, the impact on migrant wage demands is well established in the sociological literature, as part of a debate over whether minorities work in segregated occupations<sup>4</sup> or compete directly with incumbent workers (e.g. Hodge and Hodge, 1965; Snyder and Hudis, 1976; Catanzarite, 2002).

## Contributions

This study makes two main contributions. Firstly, I show how this result can be sustained in an equilibrium model. And secondly, I provide indirect empirical evidence from variation across US cities that this reservation wage effect has (1) depressed the wages of low skilled natives and (2) resulted in lower rates of native employment.

The model builds on Albrecht and Axell (1984), who study the wage and unemployment implications of heterogeneous leisure values, in the context of wage-posting monopsonistic firms. I consider an economy with standard CES technology over multiple skill factors, but within each skill group, I allow for two worker types - natives and migrants - with different reservation wages. I assume natives and migrants are equally productive and perfectly substitutable in production within skill groups<sup>5</sup>, and that there are constant (and unchanging) returns to labour within firms. Migrants may have lower reservation values for two reasons: (1) lower out-of-work utility: whether due to higher search costs (arising from language barriers, exclusion from social networks or unauthorised status), ineligibility for social transfers, binding visa requirements or other risks associated with unauthorised status; or (2) heavier discounting: many migrants intend to work for only a limited period in the host country (see Dustmann and Weiss, 2007). Realistically, many of these factors are more likely to pertain to lower skilled workers.

Random matching makes labour markets “thin”, and this is the source of firms’ monopsony power<sup>6</sup>. In contrast to Albrecht and Axell (1984), I also assume workers are subject to a continuous distribution of random job match utilities. This ensures that some natives (with high quality matches) will be willing to accept the low wages on offer to migrants. As a result, increasing the share of migrants in the economy causes native wages to fall. Of course, unlike in a competitive model, the effect on native welfare is not merely restricted to wages. Since the value of natives’ outside options declines, they are more likely to accept jobs that yield lower job match utilities. Throughout, I assume that firms cannot negotiate wage bargains with individual workers or discriminate between natives and migrants. This

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<sup>4</sup>For example, Nanos and Schluter (2013) assume that migrants and natives participate in independent labour market segments.

<sup>5</sup>Of course, this is not a true description of reality. But, the aim of the model is to show that wage effects can arise from differences in reservation values alone.

<sup>6</sup>Of course, a simple assumption of random matching does not address the root causes of monopsony power (see Manning, 2003, for a discussion of these issues). However, the aim of this study is to demonstrate the *implications* of monopsony for migrant wage effects, rather than to study the *origins* of monopsony power itself. Given this, the random matching assumption is an attractive simplification.



is clearly an extreme case; but, as long as migrants compete with natives to some extent, natives will lose part of their rents from employment.

Thus, workers lose rents through wage cuts. But who benefits? To the extent that new firms are free to enter, profits cannot be larger in equilibrium. In my model, I assume firms are free to enter at a fixed cost. So, the extra surplus is spent on the entry costs of new firms. I also set out a variant of the model with heterogeneous firms. In this case, if firm quality is in limited supply, the new entrants will be relatively less productive; and this will also serve to exhaust the surplus.

Consumers are also likely to benefit from the decline of workers' rents. In certain models of the product market (with Bertrand competition being an extreme case), firms will cut prices in response to the lower labour costs. Indeed, Cortes (2008) finds that prices of low skilled services have fallen in cities with rapidly expanding migrant populations. She interprets these effects in the context of rising low skilled output within a competitive model. But, as I explain below, I dispute her claim of minimal geographical displacement. And consequently, I would interpret her findings as reflecting falling labour costs driven by low migrant wage demands, rather than changes in the supply of these services.

My second contribution is to provide indirect empirical evidence for this phenomenon, based on variation across US cities. The model suggests that native earnings in a given skill group are a function of (1) the skill-specific supply of labour (which affects the marginal product) and (2) the migrant share (which affects wage demands). I wish to study the effect of migrant share, holding skill-specific labour supply fixed. Of course, migrant share is somewhat endogenous to wages. A natural instrument in a cross-city setting, initially popularised by Card (2001), is based on migrant enclaves. Specifically, I estimate the predicted contribution of migration to local population growth, assuming that new migrants of each origin country settle in different areas proportionally with the initial geographical distribution of their co-patriot communities.<sup>7</sup> And indeed, the instrument has a strong effect on the local migrant share - with most of the impact felt in low skilled markets, where migrants are overrepresented.<sup>8</sup>

The empirical challenge here is then to deal with the endogeneity of labour supply. Controlling for labour supply directly is not a viable option, because it itself is endogenous to wages. My approach instead is simply to omit labour supply as a control. This may at first appear equally unappealing: the instrument's exclusion restriction will be violated if areas with larger migrant enclaves experience growing labour supply as well as migrant share. However, I present new evidence of substantial displacement of low skilled natives and longer term migrants from large enclave areas - to the extent that the local supply of low and high skilled labour are little changed, at least until the 2000s. Of course, at the same time, the large native displacement greatly amplifies the effect on migrant share in low skilled markets. This is a significant finding, because the question of displacement has

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<sup>7</sup>It is well known that migrants tend to cluster in those cities where their communities have historically settled, whether because of job networks (Munshi, 2003) or cultural amenities (Gonzalez, 1998).

<sup>8</sup>This is despite the commonly cited fact that a similar share of migrants and natives hold college degrees. See, for example, Card (2009b) or Ottaviano and Peri (2012).

long been the subject of vigorous debate<sup>9</sup>. But, I argue that those studies which dispute displacement are usually compromised by restrictive skill group definitions. Instead, I take a flexible approach, reporting estimates separately by two-digit occupations.

Despite this large displacement, I still identify large negative effects on low skilled native wages (where skill is defined by occupation) - with the bulk of the effect coming in the 1970s, at the same time as a rapid acceleration of immigration. In that decade, controlling for local demand shock predictors, I predict that immigration triggered an 8 log point decline in local native wages in the lowest skilled jobs in a popular migrant destination like New York; but the effect on high skilled wages was minimal. There may be concerns that the that local wage changes reflect sorting of different types of workers across cities. But, I reject this explanation after comparing the wages of current and previous residents of each city. Instead, I argue that the depressing effect of low migrant reservation wages is a plausible explanation. Of course, this is only one component of the overall wage effect of migration: changes in marginal products cannot be identified using cross-city variation precisely because of large geographical displacement (as argued by Borjas et al., 1997).

While observed wages did not respond much to migration in subsequent decades, it is still plausible that the wage offer distribution shifted downwards. This is because I find large effects on low skilled native employment-population ratios in the 1980s and 1990s. This growing number of jobless natives are likely to be rejecting wage offers at the bottom of the distribution. And since these offers are unobserved, wage offers may well be growing more slowly than observed wages in the affected cities. This result has important implications for the interpretation of empirical studies in the US migration literature. Several of these studies (such as Borjas, 2006; Cortes, 2008) are estimated on data *after* the 1970s, when the bulk of the local native wage effect materialised. Such an approach will miss important wage effects, if selection through employment rates is not acknowledged.

In the following section, I set out an equilibrium model that illustrates how migrant reservation values affect native wage and employment outcomes. Section 2.3 shows how the model's predictions can be tested with city-level data. I describe the data in Section 2.4 and discuss the relative merits of education and occupation-based measures of skill. I also report some evidence on the power of the enclave instrument. The main empirical results are presented in Section 2.5, and Section 2.6 concludes with a discussion of policy implications.

## 2.2 Model

### Overview

I present a simple discrete-time job matching model with wage-setting monopsonistic firms. Labour is the sole factor of production. There are  $n_s$  workers of skill  $s$ , of whom  $n_s^M = \lambda_s n_s$  are migrants and  $n_s^N = (1 - \lambda_s) n_s$  are natives. For simplicity, I assume natives and migrants

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<sup>9</sup>To list a few examples, Frey (1995; 1996), Borjas et al. (1997) and Borjas (2006) provide evidence in favour of geographical displacement; whereas Card (2001; 2009a), Cortes (2008) and Peri and Sparber (2011) provide evidence against.

are perfect substitutes within skill group  $s$ . The labour market is fully segmented by skill, with workers of skill  $s$  employed exclusively to produce an intermediate good  $y_s$  (priced at  $p_s$ ). Intermediate goods are combined to create a final good  $y$  (whose price is normalised to 1) using a CES technology. The market for final goods production is competitive: for each skill type  $s$ ,  $p_s$  is set equal to the marginal contribution of  $y_s$  to the final good. But, labour markets are not competitive due to random matching between workers and intermediate goods firms.

For each skill type  $s$ , there are  $k_s$  intermediate firms, denoted  $j$ . There are constant returns to labour within each firm, and I assume that each worker of skill  $s$  produces one unit of the intermediate good  $y_s$ . In an extension below, I allow for heterogeneity in worker productivity across firms within skill groups. Firms choose whether to be active or inactive; they must pay a one-off cost of  $c$  if they decide to enter the market. I denote the share of active firms in market  $s$  as  $\theta_s$ . Firms set wages to maximise profits. I assume that  $\theta_s k_s$  is large, so individual firms take other firms' choices as exogenous when making their own wage decision. Also, I assume firms cannot discriminate in wage offers between natives and migrants. This is clearly a strong assumption, but as long as labour markets are not entirely segregated between natives and migrants, the qualitative results outlined below will hold.

Firms exist forever, but workers face a probability  $\delta^T$  of exiting the labour market in any period (after which they receive zero utility forever), where  $\delta^T$  varies across worker types  $T = \{M, N\}$ .  $\delta^T$  also functions as the worker's discount rate in this model. The worker population in skill group  $s$  is maintained by a constant flow of  $\delta^N (1 - \lambda) n_s$  natives and  $\delta^M \lambda n_s$  migrants entering the labour market. Each period, new workers of skill  $s$  draw a wage  $w_j$  from the endogenous offer distribution  $F_s^w$ ; they face an equal probability  $\frac{1}{\theta k_s}$  of meeting any given firm. Simultaneously, workers draw a random job match utility parameter  $\varepsilon_i$ , where  $\log \varepsilon_i$  has an exogenous symmetric distribution  $F^\varepsilon$  with mean 0. I assume the cumulative density function  $F^\varepsilon(\varepsilon)$  is continuous and differentiable over the full support of  $\varepsilon$  and that its density function  $f^\varepsilon(\varepsilon)$  is also continuous and differentiable. I also assume that the hazard rate of  $F^\varepsilon$  (i.e.  $\frac{f^\varepsilon(\varepsilon)}{1 - F^\varepsilon(\varepsilon)}$ ) is monotonically increasing over the full support of  $\varepsilon$ .<sup>10</sup>

If workers reject a job offer, they receive a leisure flow  $b^T > 0$  and survive with probability  $1 - \delta^T$  to draw a new wage offer. Once they accept a job offer, they remain at the same firm until they exit the labour market, receiving a flow utility of  $\varepsilon_i w_j$  each period. In setting their wage  $w_j$ , firms trade off profit per worker with labour force size.

In this model, migrants may have relatively low reservation wages for two reasons: (1) smaller leisure values,  $b^M < b^N$  or (2) heavier discounting,  $\delta^M > \delta^N$ . If either of these conditions are true, I show that an increase in the migrant share  $\lambda$  will reduce natives' reservation values, their equilibrium wage and their employment rate, given some marginal product  $y_s$ . This result is entirely dependent on firms having market power. In the competitive world which characterises most of the migration literature, any wage effect of migration can only come through the marginal product. The aim of this exercise is to study the qualitative results: I do not attempt to calibrate the model. In Section 2.3, I provide empirical evidence for this reservation wage channel using geographical variation in the US.

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<sup>10</sup>This is true for most standard distributions, including the normal distribution.

The model is closely related to Albrecht and Axell (1984), who study the labour market implications of workers with heterogeneous leisure values. In my model, within each skill group  $s$ , there are two worker types  $T_s = \{N_s, M_s\}$ , i.e. natives and migrants, who share the same productivity but differ in preference and job search parameters. Their key departure from Albrecht and Axell is heterogeneous preferences over job matches, beyond the mere wage offer: this ensures that wages do not collapse to the value of leisure in equilibrium.

## Final good production

The final output good  $y$  is produced according to the following CES technology:

$$y = \left( \sum_s \alpha_s y_s^\sigma \right)^{\frac{1}{\sigma}} \quad (2.1)$$

where  $\sigma \in [-\infty, 1]$ .  $y_s$  is the output of intermediate good  $s$ , and  $\sum_s \alpha_s = 1$ . I assume the final goods market is competitive, so the price  $p_s$  of each good is set equal to the marginal product:

$$p_s = \alpha_s \left( \frac{y}{y_s} \right)^{1-\sigma} \quad (2.2)$$

## Intermediate goods firms

There is a limited supply of firms  $k_s$  in the market for good  $s$ , of which a fraction  $\theta_s$  are active. Firms in market  $s$  employ a single factor (labour of skill  $s$ ) with constant returns to scale: each worker of skill  $s$  produces a single unit of good  $s$  each period. A given active firm  $j$  in market  $s$  maximises its profit  $\pi_{sj}$  by setting its wage offer  $w_j$ . The firm's wage-setting problem is characterised as follows:

$$\max_{w_j} \pi_s(w_j) = (p_s - w_j) l_s(w_j) \quad (2.3)$$

where  $l_s(w_j)$  is the firm's labour force. Assuming individual firms are small relative to the market for good  $s$ , firms take the price  $p_s$  as given.

I define  $l_s^T(w_j)$  as the number of  $T$  type workers of skill  $s$  employed by firm  $j$ , where  $l_s^N(w_j) + l_s^M(w_j) = l_s(w_j)$ . The steady-state level of  $l_s^T(w_j)$  can be derived from the following equation:

$$\frac{1}{\theta_s k_s} \mu_s^T(w_j) u_s^T = \delta^T l_s^T(w_j) \quad (2.4)$$

where the left hand side is the the number of type  $T$  workers entering employment in firm  $j$  next period, and the right hand side is the number of workers leaving.  $u_s^T$  as the number of type  $T$  skill  $s$  workers currently searching for work.  $\frac{1}{\theta_s k_s}$  is the probability that a worker meets firm  $j$ , and  $\mu_s^T(w_j)$  is the probability that a type  $T$  worker of skill  $s$  accepts the job

offer  $w$  (i.e. the labour supply equation). The outflow is the product of  $l_s^T(w_j)$  and the probability of leaving the labour force,  $\delta^T$ .

To solve for  $l_s^T(w_j)$ , I need a steady-state expression for  $u_s^T$ . Equating outflows from the stock of searching workers  $u_s^T$  with inflows gives:

$$\delta^T u_s^T + (1 - \delta^T) \int_w \mu_s^T(w) dF_s^w \cdot u_s^T = \delta^T n_s^T \quad (2.5)$$

On the left hand side of equation (2.5), I allow for two ways to exit the job search state: the first term ( $\delta^T u_s^T$ ) represents leaving the labour force; the second represents finding a job, where  $1 - \delta^T$  is the fraction of searching workers who remain in the labour force, and  $\int_w \mu_s^T(w) dF_s^w$  is the unconditional probability of accepting an offer next period. The term  $\delta^T n_s^T$  is the number of type  $T$  workers entering the search state. Rearranging equation (2.5) for  $u_s^T$ , and substituting this into equation (2.4) gives:

$$l_s^T(w_j) = \frac{\frac{1}{\theta_s k_s} \mu_s^T(w_j) n_s^T}{(1 - \delta^T) \int_w \mu_s^T(w) dF_s^w + \delta^T} \quad (2.6)$$

Now, substituting the solutions for  $l_s^N(w_j)$  and  $l_s^M(w_j)$  into the firm's problem above yields:

$$\max_{w_j} \pi_s(w_j) = (p_s - w_j) \frac{n_s}{\theta_s k_s} \sum_T \frac{\lambda_s^T \mu_s^T(w_j)}{(1 - \delta^T) \int_w \mu_s^T(w) dF_s^w + \delta^T} \quad (2.7)$$

where I define  $\lambda_s^N = 1 - \lambda_s$  as the share of natives, and  $\lambda_s^M = \lambda_s$  as the share of migrants. Since firms take  $\int_w \mu_s^T(w) dF_s^w$  and  $\theta_s$  as given (there are many firms), the first order condition is:

$$p_s - w_j = \frac{(1 - \lambda) \mu_s^N(w_j) + \lambda_s q(F_s^w, v_s^N, v_s^M) \mu_s^M(w_j)}{(1 - \lambda) \mu_s^{N'}(w_j) + \lambda_s q(F_s^w, v_s^N, v_s^M) \mu_s^{M'}(w_j)} \quad (2.8)$$

where

$$q(F_s^w, v_s^N, v_s^M) = \frac{(1 - \delta^N) \int_w \mu_s^N(w) dF_s^w + \delta^N}{(1 - \delta^M) \int_w \mu_s^M(w) dF_s^w + \delta^M} \quad (2.9)$$

Finally, let  $\pi_s^*(\theta_s, v_s^N, v_s^M, F_s^w)$  denote the indirect profit function, given the optimal wage  $w_s^*(v_s^N, v_s^M, F_s^w)$ . Firms will become active until  $\pi_s^*(\theta_s, v_s^N, v_s^M, F_s^w) = c$ .

## Workers

A worker of skill  $s$  and type  $T = \{M, N\}$  accepts a job offer with flow utility  $\varepsilon_i w_j$  if  $\frac{\varepsilon_i w_j}{\delta^T} > V_s^T$ , where  $V_s^T$  is the value of searching for work.  $V_s^T$  is characterised as follows:

$$V_s^T = b^T + (1 - \delta^T) \int_w \int_{\varepsilon} \max \left\{ \frac{\varepsilon w}{\delta^T}, R_s^T \right\} dF_s^{\varepsilon} dF_s^w \quad (2.10)$$

Equivalently, this can be expressed in terms of the discounted value of searching,  $v_s^T = \delta^T R_s^T$ :

$$v_s^T = \delta^T b^T + (1 - \delta^T) \int_w \int_\varepsilon \max \{ \varepsilon w, v_s^T \} dF^\varepsilon dF_s^w \quad (2.11)$$

Notice that  $v_s^T$  is increasing in the leisure flow  $b^T$ . And, as long as the discounted value of searching  $\int_w \int_\varepsilon \max \{ \varepsilon w, v_s^T \} dF^\varepsilon dF_s^w$  exceeds  $b^T$  (as seems plausible for the average worker),  $v_s^T$  is decreasing in  $\delta^T$ . The distribution of preferences over matches  $F^\varepsilon$  is exogenous, but the distribution of wage offers  $F_s^w$  is endogenous to firm decisions.

Based on the above,  $\mu_s(w_j)$ , the probability that a worker of skill  $s$  accepts a wage offer  $w_j$ , can be expressed as follows:

$$\begin{aligned} \mu_s(w_j) &= \Pr \left( \log \varepsilon_i > \log \frac{v_s^T}{w_j} \right) \\ &= \Pr \left( \log \varepsilon_i < \log \frac{w_j}{v_s^T} \right) \\ &= F^\varepsilon \left( \log \frac{w_j}{v_s^T} \right) \end{aligned} \quad (2.12)$$

assuming that  $\log \varepsilon$  is symmetrically distributed with mean 0.

## Equilibrium

Combining the equations for wage-setting (equation (2.8)) and labour supply (equation (2.12)) yields an expression for the proportional markup:

$$\frac{p_s - w_j}{w_j} = \frac{(1 - \lambda_s) F^\varepsilon \left( \log \frac{w_j}{v_s^N} \right) + \lambda_s q \left( F_s^w, v_s^N, v_s^M \right) F^\varepsilon \left( \log \frac{w_j}{v_s^M} \right)}{(1 - \lambda_s) f^\varepsilon \left( \log \frac{w_j}{v_s^N} \right) + \lambda_s q \left( F_s^w, v_s^N, v_s^M \right) f^\varepsilon \left( \log \frac{w_j}{v_s^M} \right)} \quad (2.13)$$

Based on the monotone hazard properties assumed for  $F^\varepsilon$ , the right hand side of equation (2.13) must be an increasing function of  $w$ .<sup>11</sup> As a result, since the left hand side is decreasing in  $w$ , there is a unique solution for the wage  $w_s$  for skill group  $s$ , given  $v_s^N$ ,  $v_s^M$  and  $F_s^w$ .

Since the wage outcome in equation (2.13) is unique, given  $v_s^N$ ,  $v_s^M$  and  $F_s^w$ , all firms set the same wage in equilibrium. And so,  $F_s^w$  collapses to a unit mass at  $w_s$ . Then, assuming that  $\delta_s^N$  and  $\delta_s^M$  are small relative to the job acceptance probabilities  $\mu_s^M(w_j)$  and  $\mu_s^N(w_j)$ , the function  $q$  in equation (2.9) can be approximated as:

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<sup>11</sup>To see this, define  $H_s^T(w) = F^\varepsilon \left( \log \frac{w}{v_s^T} \right)$  and  $H_s(w) = (1 - \beta) H_s^N(w) + \beta H_s^M(w)$ , where  $H_s(w)$  is a mixture of two distributions  $H_s^N(w)$  and  $H_s^M(w)$ , weighted by  $\beta = \frac{\lambda_s q(F_s^w, v_s^N, v_s^M)}{1 - \lambda_s + \lambda_s q(F_s^w, v_s^N, v_s^M)}$ . The right hand side of equation (2.13) can then be expressed as  $\frac{H_s(w)}{h_s(w)}$ . Notice that both  $\frac{H_s^N(w)}{h_s^N(w)}$  and  $\frac{H_s^M(w)}{h_s^M(w)}$  are both increasing monotonically in  $w$  (a consequence of the monotone hazard rate assumption on  $F^\varepsilon$ ). And so, the same must be true for  $\frac{H_s(w)}{h_s(w)}$ .

$$q(F_s^w, v_s^N, v_s^M) \approx \frac{F^\varepsilon \left( \log \frac{w_s}{v_s^N} \right)}{F^\varepsilon \left( \log \frac{w_s}{v_s^M} \right)} \quad (2.14)$$

Substituting  $q$  into the firm's first order condition, equation (2.13), and rearranging:

$$\frac{w_s}{p_s - w_s} = (1 - \lambda_s) \frac{f^\varepsilon \left( \log \frac{w_s}{v_s^N} \right)}{F^\varepsilon \left( \log \frac{w_s}{v_s^N} \right)} + \lambda_s \frac{f^\varepsilon \left( \log \frac{w_s}{v_s^M} \right)}{F^\varepsilon \left( \log \frac{w_s}{v_s^M} \right)} \quad (2.15)$$

which gives the equilibrium wage, as a function of natives' and migrants' reservation values.

There are three other equilibrium conditions. The value of rejecting job offers for natives and migrants respectively are:

$$v_s^T = \delta^T b^T + (1 - \delta^T) \int_{\varepsilon} \max \{ \varepsilon w_s, v_s^T \} dF^\varepsilon, \quad T = \{M, N\} \quad (2.16)$$

Equations (2.15) and (2.16) are sufficient to solve for  $w_s$ ,  $v_s^N$  and  $v_s^M$ . Given the solution for these three unknowns, the fourth and final condition (arising from free entry) fixes the share of active firms  $\theta$ :

$$(p_s - w_s) \frac{n_s}{\theta_s k_s} \sum_T \frac{\lambda_s^T F^\varepsilon \left( \log \frac{w_s}{v_s^T} \right)}{(1 - \delta^T) F^\varepsilon \left( \log \frac{w_s}{v_s^T} \right) + \delta^T} = c \quad (2.17)$$

This equation imposes that firm profits are equal to the activation cost  $c$ .

## Solution

Notice that the first three equilibrium conditions (contained in equations (2.15) and (2.16)) are sufficient to solve for  $v_s^N$ ,  $v_s^M$  and  $w_s$ , independently of  $\theta_s$ . Firm entry drives profit to zero, but does not affect outcomes for workers in this model.

Equation (2.16) shows that the rejection value  $v_s^T$  is bounded below at  $b^T$  when  $w_s = 0$  and is increasing in  $w_s$  thereafter. The gradient of  $v_s^T$  with respect to  $w$  is never greater than one.<sup>12</sup> Equation (2.15) shows how the firm's mark-up relates to the distribution of workers' outside options. The mark-up is increasing in  $v_s^N$  and  $v_s^M$ . The firm's wage choice goes to zero as  $v_s^N$  and  $v_s^M$  tend to zero. And, if  $\lambda_s$  is small,  $w_s$  grows more than one-for-one with  $v_s^N$ .<sup>13</sup> The properties of these equations guarantee the existence of a unique equilibrium in  $v_s^N$ ,  $v_s^M$  and  $w_s$ .

<sup>12</sup>To see this, suppose that  $v_s^T$  and  $w_s$  both grow at rate  $\zeta$ . The term  $\int_{\varepsilon} \max \{ \varepsilon w_s, v_s^T \}$  therefore also grows at rate  $\zeta$ . But then, given  $\delta^T < 1$  and  $b^T > 0$ , the right hand side of equation (2.16) must grow slower than  $\zeta$ , despite the left hand side growing at  $\zeta$ . To keep the equation in balance, it must be that  $v_s^T$  grows more slowly than  $w_s$ .

<sup>13</sup>To see this, suppose that  $\lambda_s = 0$ , and that  $v_s^N$  and  $w_s$  both grow at rate  $\zeta$ . The right hand side of equation (2.15) will remain unchanged, but the left hand side will shrink. And so, to keep the equation in balance,  $w_s$  must grow faster than  $v_s^N$ .

The key point of interest is the impact of immigration on  $w_s$  and  $v_s^N$ . An increase in  $\lambda_s$  enters the model through its impact on firms' wage decisions. I assume migrants have lower rejection values ( $v_s^M < v_s^N$ ), whether because of lower leisure values ( $b^M < b^N$ ) or higher discount rates ( $\delta^M > \delta^N$ ). Thus, firms will exploit an increase in the skill-specific migrant share  $\lambda_s$  by cutting wages and expanding their mark-ups. This intuition can be appreciated

in equation (2.16). The expression  $\frac{f^\varepsilon\left(\log \frac{w_s}{v_s^T}\right)}{F^\varepsilon\left(\log \frac{w_s}{v_s^T}\right)}$  is the density of type  $T$  workers, conditional on accepting the wage offer  $w_s$ , who are on the margin of rejecting. When this conditional density is larger, workers have greater wage-setting power, and the wage equilibrium will be closer to the marginal product  $p_s$ . (Notice that as the conditional density tends to infinity, the wage outcome  $w_s$  converges to  $p_s$ .) Now, based on the monotone hazard rate assumption on  $F^\varepsilon$ , the conditional density of natives must be larger than that of migrants.<sup>14</sup> And consequently, the equilibrium wage must be decreasing in the migrant share  $\lambda_s$ , conditional on the marginal product  $p_s$ .

Clearly, a decline in the wage  $w_s$  will cause the native rejection value  $v_s^N$  to fall. But, based on the argument above, the rejection value will decline proportionately less, so  $\frac{w_s}{v_s^N}$  decreases. Consequently, a smaller fraction of natives will accept job offers:  $F^\varepsilon\left(\log \frac{w_s}{v_s^N}\right)$  falls. Or equivalently, the native employment rate declines.

It is instructive to compare the equilibrium of this model with that of Albrecht and Axell (1984). The framework is very similar: firms set wages to maximise profit, and there are two worker types with high and low leisure values (equivalent to my “natives” and “migrants” respectively). However, they do not include heterogeneity in job match preferences. As a result, a growing share of low value workers has no effect on the wages of high value workers. In equilibrium, firms that wish to recruit these workers set wages at exactly their reservation value. There is no incentive to reduce the wage below this point - even following an influx of migrants - because high value workers will always decline such offers. In contrast, job match heterogeneity gives firms the flexibility to adjust their offer: the job acceptance probability is no longer on a knife-edge, but is a continuous function of wages. A larger migrant share will cause a fall wages, as firms face more workers with lower reservation values.

## Extension with heterogeneous firms

Suppose that firms are heterogeneous in their productivity: an employee of firm  $j$  produces  $\gamma_j$  units of the output good (rather than simply one unit in all firms, as assumed above), where  $\gamma_j$  varies across the  $k$  firms according to some distribution  $F^\gamma$ . Effectively, there is a limited supply of productive firms. The result will look similar to the homogeneous firm case, except that firm entry (for example, where triggered by a larger migrant population) will be accompanied by a declined in average firm productivity as the operations of lower

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<sup>14</sup>Specifically, since  $v_s^M < v_s^N$ , it must be that  $\frac{f^\varepsilon\left(\log \frac{w_s}{v_s^M}\right)}{F^\varepsilon\left(\log \frac{w_s}{v_s^M}\right)} < \frac{f^\varepsilon\left(\log \frac{w_s}{v_s^N}\right)}{F^\varepsilon\left(\log \frac{w_s}{v_s^N}\right)}$ .



quality firms become viable. Furthermore, all but the marginal firm will receive positive profits in equilibrium.

Equilibrium in each market  $s$  can be characterised as five unknowns  $\{v_s^N, v_s^M, F_s^w, \theta_s, \bar{\gamma}_s\}$  together with the following equations, where  $\bar{\gamma}_s$  is the productivity of the marginal firm. The discounted value of rejecting job offers, for natives and migrants respectively, is:

$$v_s^T = \delta^T b^T + (1 - \delta^T) \int_w \int_\varepsilon \max \{ \varepsilon w, v_s^T \} dF^\varepsilon dF_s^w, \quad T = \{M, N\} \quad (2.18)$$

The wage offer distribution can be derived by integrating over the distribution of firm productivities, above the marginal productivity  $\bar{\gamma}_s$ :

$$F_s^w(x) = \int_{\bar{\gamma}_s}^x w^*(\gamma, v_s^N, v_s^M) dF^\gamma \quad (2.19)$$

where  $w^*(\gamma, v_s^N, v_s^M)$  is the implicit function based on the firm's wage-setting first order condition:

$$\frac{p_s \gamma - w^*}{w^*} = \frac{(1 - \lambda_s) F^\varepsilon \left( \log \frac{w^*}{v_s^N} \right) + \lambda_s q(F_s^w, v_s^N, v_s^M) F^\varepsilon \left( \log \frac{w^*}{v_s^M} \right)}{(1 - \lambda_s) f^\varepsilon \left( \log \frac{w^*}{v_s^N} \right) + \lambda_s q(F_s^w, v_s^N, v_s^M) f^\varepsilon \left( \log \frac{w^*}{v_s^M} \right)} \quad (2.20)$$

where

$$q(F_s^w, v_s^N, v_s^M) = \frac{(1 - \delta^N) \int_w F^\varepsilon \left( \log \frac{w}{v_s^N} \right) dF_s^w + \delta^N}{(1 - \delta^M) \int_w F^\varepsilon \left( \log \frac{w}{v_s^M} \right) dF_s^w + \delta^M} \quad (2.21)$$

Free entry guarantees that the profit of the marginal firm is equal to the activation cost  $c$ :

$$\left[ \bar{\gamma}_s - w^*(\bar{\gamma}_s, v_s^N, v_s^M) \right] \frac{n_s}{\theta_s k_s} \sum_T \frac{\lambda_s^T F^\varepsilon \left( \log \frac{w^*(\bar{\gamma}_s, v_s^N, v_s^M)}{v_s^T} \right)}{(1 - \delta^T) \int_w F^\varepsilon \left( \log \frac{w}{v_s^T} \right) dF_s^w + \delta^T} = c \quad (2.22)$$

where

$$\theta_s = 1 - F^\gamma(\bar{\gamma}_s) \quad (2.23)$$

Based on equation (2.20), lower quality firms (with lower  $\gamma$ ) will set a lower wage  $w^*$  in equilibrium. As a result, they will recruit fewer workers. A positive relationship between firm size and wages is well documented in the literature (e.g. Brown and Medoff, 1989; Oi and Idson, 1999). The idea that this correlation (partly) reflects a monopsonistic employer's upward-sloping labour supply curve is not new (e.g. Weiss and Landau, 1984; Manning, 2003).

## 2.3 Empirical application

### Multi-city model

Below, I test the predictions of the model using cross-city variation. To begin, I extend the model above to incorporate multiple cities or regions  $r$ . Each city  $r$  produces a distinct final good  $y_r$  based on the CES technology outlined above:

$$y_r = z_r \left( \sum_{sr} \alpha_{sr} y_{sr}^\sigma \right)^{\frac{1}{\sigma}} \quad (2.24)$$

which aggregates intermediate goods  $y_{sr}$  produced in that city  $r$ . For simplicity, I assume the price of the final good  $y_r$  is fixed in international markets. If  $y_r$  is understood to be the nominal output of the final good, then the aggregate shifter  $z_r$  can represent either price or productivity shocks. Given that each worker of skill  $s$  produces one intermediate good, the marginal product of such a worker in city  $r$  is:

$$p_{sr} = \alpha_{sr} z_r \left( \frac{\bar{n}_r}{n_{sr}} \right)^{1-\sigma} \quad (2.25)$$

where  $n_{sr}$  is the local supply of workers of skill  $s$ , and  $\bar{n}_r = (\sum_{sr} \alpha_{sr} n_{sr}^\sigma)^{\frac{1}{\sigma}}$  is a CES aggregator across employment in all local skill groups. Substituting this into the firm's first order condition, equation (2.15), yields:

$$w_{sr} = \alpha_{sr} z_r \left( \frac{\bar{n}_r}{n_{sr}} \right)^{1-\sigma} \left[ \frac{(1 - \lambda_{sr}) g_{sr}^N + \lambda_{sr} g_{sr}^M}{(1 - \lambda_{sr}) g_{sr}^N + \lambda_{sr} g_{sr}^M + 1} \right] \quad (2.26)$$

where

$$g_{sr}^T = \frac{f^\varepsilon \left( \log \frac{w_{sr}}{v_{sr}^T} \right)}{F^\varepsilon \left( \log \frac{w_{sr}}{v_{sr}^T} \right)} \quad (2.27)$$

is the conditional density for type  $T$  workers. And finally, the unemployment value in city  $r$  is:

$$v_{sr}^T = (1 - \mu) \left[ \delta^T b^T + (1 - \delta^T) \int_{\varepsilon} \max \{ a_r^T + \varepsilon w_{sr}, v_{sr}^T \} dF^\varepsilon \right] + \mu \max_r \{ v_{sr}^T \}, \quad T = \{M, N\} \quad (2.28)$$

where, each period, workers have the opportunity with probability  $\mu$  of moving to the city with the largest unemployment value. This approach of allowing for mobility follows Beaudry et al. (2013).

## Linear approximation

To derive the estimating equation, I take a first order approximation of the equilibrium conditions. Firstly, for small migrant share  $\lambda_{sr}$ , the wage-setting curve (equation (2.26)) can be approximated as<sup>15</sup>:

$$\hat{w}_{sr} = \hat{\alpha}_{sr} + \hat{z}_r - (1 - \sigma) (\hat{n}_{sr} - \hat{n}_r) - \frac{1}{g_{sr}^N} - \left[ \frac{g_{sr}^N - g_{sr}^M}{g_{sr}^N (1 + g_{sr}^N)} \right] \lambda_{sr} \quad (2.29)$$

where  $\hat{x}$  denotes the logarithm of  $x$ . The wage approaches the marginal product as the conditional density for both natives and migrants approach 1. Since  $g_{sr}^N > g_{sr}^M$ , the wage is decreasing in  $\lambda_{sr}$ .

Of course, the conditional densities  $g_{sr}^T$  are themselves a function of wages. Notice first that, based on the labour supply curve (equation (2.28)) - and as argued above, the ratio  $\frac{w_{sr}}{v_{sr}^T}$  is increasing in the wage. So, given the monotone hazard rate assumption, the conditional density  $g_{sr}^T$  must be decreasing in  $w_{sr}$  (see equation (2.27)). That is, conditional on accepting the wage offer  $w_{sr}$ , fewer workers are on the margin of rejecting as the wage increases. This grants firms more pricing power, so the markup grows with the marginal product. Assuming a linear relation in the inverse of  $g_{sr}^T$  gives:

$$\frac{1}{g_{sr}^T} = \zeta_0^T + \zeta_1^T \hat{w}_{sr} \quad (2.30)$$

where  $\zeta_1^T > 0$ . Since I assume  $\lambda_{sr}$  is small, the feedback effects through the  $\left[ \frac{g_{sr}^N - g_{sr}^M}{g_{sr}^N (1 + g_{sr}^N)} \right]$  term will also be small; so I assume that term is fixed at  $\zeta_2$  for simplicity. Then, substituting the labour supply relationship (equation (2.30)) into the wage-setting curve yields:

$$\hat{w}_{sr} = \frac{1}{1 + \zeta_1^N} \left\{ -\zeta_0^N + \hat{\alpha}_{sr} + \hat{z}_r - (1 - \sigma) (\hat{n}_{sr} - \hat{n}_r) - \zeta_2 \lambda_{sr} \right\} \quad (2.31)$$

And taking first differences:

$$\hat{w}_{sr} = \phi_1 \Delta \hat{\alpha}_{srt} + \phi_2 \Delta \hat{z}_{rt} + \phi_3 (\Delta \hat{n}_{srt} - \Delta \hat{n}_{rt}) + \phi_4 \Delta \lambda_{srt} \quad (2.32)$$

where  $\Delta$  denotes a decadal change between census years  $t$ .

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<sup>15</sup>The proof is as follows. Equation (2.26) can be rearranged as:

$$w_{sr} = \alpha_{sr} \bar{z}_r \left( \frac{\bar{n}_r}{n_{sr}} \right)^{1-\sigma} \left( \frac{g_{sr}^N}{1 + g_{sr}^N} \right) \left[ \frac{1 - \lambda_{sr} \left( \frac{g_{sr}^N - g_{sr}^M}{g_{sr}^N} \right)}{1 - \lambda_{sr} \left( \frac{g_{sr}^N - g_{sr}^M}{1 + g_{sr}^N} \right)} \right]$$

And in logarithmic terms:

$$\hat{w}_{sr} = \hat{\alpha}_{sr} + \hat{z}_r - (1 - \sigma) (\hat{n}_{sr} - \hat{n}_r) - \log \left( 1 + \frac{1}{g_{sr}^N} \right) + \log \left[ 1 - \lambda_{sr} \left( \frac{g_{sr}^N - g_{sr}^M}{g_{sr}^N} \right) \right] - \log \left[ 1 - \lambda_{sr} \left( \frac{g_{sr}^N - g_{sr}^M}{1 + g_{sr}^N} \right) \right]$$

And, for small  $g_{sr}^N$  and  $\lambda_{sr}$ , this approximates the expression in equation (2.29).

## Theoretical challenges

Equation (2.32) describes the equilibrium response of the local wage in skill group  $s$  to (1) skill biases in technology represented by  $\hat{\alpha}_{sr}$ , (2) the local aggregate productivity shifter or trade price  $\hat{z}_{rt}$ , (3) the local relative supply of the labour skill factor  $(\hat{n}_{sr} - \hat{n}_r)$ , and (4) the skill-specific local migrant share  $\lambda_{sr}$ . Assuming the model is correct, the marginal product is fully determined by  $\hat{\alpha}_{sr}$ ,  $\hat{z}_{sr}$  and  $(\hat{n}_{sr} - \hat{n}_r)$ . And consequently, with the other variables held fixed, any effect of  $\lambda_{sr}$  on wages must enter through the mark-up. If migrants have lower wage demands than natives, this effect will be negative.

The key assumption behind this result is that, within skill groups, natives and migrants are homogeneous and perfect substitutes in production. Suppose instead that migrants and natives are imperfect substitutes. Manacorda et al., 2012 find evidence for imperfect substitutability within education groups in the UK, based on wage regressions; and Card (2009b) and Ottaviano and Peri, 2012 find similar results for the US, though Borjas et al. (2008, 2010 and 2012) dispute the US results. Also, Dustmann et al. (2013) argue that these results are sensitive to the assignment of migrants to skill groups (in particular, skill downgrading by migrants on arrival may complicate the interpretation): imperfect substitutability is probably less of a concern within detailed occupation groups. In any case, consider the following technology with imperfect substitutability:

$$y_r = z_r \left( \sum_{sr} \alpha_{sr} \bar{n}_{sr}^\sigma \right)^{\frac{1}{\sigma}} \quad (2.33)$$

where

$$\bar{n}_{sr} = \left( \phi n_{sr}^{N\rho} + (1 - \phi) n_{sr}^{M\rho} \right)^{\frac{1}{\rho}} \quad (2.34)$$

The term  $\bar{n}_{sr}$  is a CES aggregator over skill-specific native labour  $n_{sr}^N$  and migrant labour  $n_{sr}^M$ . Clearly, unlike in the model outlined above, the marginal products native and migrant labour in the same skill group are now different. Specifically, the native marginal product in skill group  $s$  and city  $r$  is:

$$p_{sr}^N = \phi \alpha_{sr} z_r \left( \frac{\bar{n}_r}{\bar{n}_{sr}} \right)^{1-\sigma} \left( \frac{\bar{n}_{sr}}{n_{sr}^N} \right)^{1-\rho} \quad (2.35)$$

What is the effect of skill-specific migrant share on  $p_{sr}^N$ ? With relative skill supplies and  $z_r$  and the aggregated skill factor share  $\frac{\bar{n}_r}{\bar{n}_{sr}}$  held constant, an increase in the migrant share  $\lambda_{sr}$  is associated with a rise in  $\frac{\bar{n}_{sr}}{n_{sr}^N}$  and an *increase* in the marginal product; and this in turn, will put upward pressure on native wages. Consequently, as migrant share expands, imperfect substitutability will counteract any downward wage effect that may arise from growing mark-ups.

Alternatively, suppose that natives and migrants are perfect substitutes ( $\rho = 1$ ), but natives are relatively more productive due to language skills or past experience in the American labour market ( $\phi > 0.5$ ). In this scenario, a rising migrant share would cause the total effective labour supply in the skill group to decline. As a result,  $\bar{n}_{sr}$  falls, and the marginal

product of workers of skill  $s$  rises. And so, as with imperfect substitutability, a native-migrant productivity gap would cause the wage effect of migrant share to be less negative, all else equal. This should be taken into account when interpreting the estimates below.

Finally, especially in markets with downward wage rigidity (perhaps due to minimum wage legislation), firms may choose to respond to migration by downgrading workplace amenities rather than native wages. Ideally, I would therefore estimate the impact of migration on native wages *net* of amenities. Unfortunately, the US census does not provide information on these workplace characteristics. So, in this sense, my gross wage estimates may understate the overall impact. Having said that, Hamermesh (1998) does not find evidence for such effects.

## Empirical challenges

Above, I have described some theoretical considerations in interpreting the skill-specific wage effect of migrant share  $\lambda_{st}$ , with  $\hat{\alpha}_{srt}$ ,  $\hat{z}_{srt}$  and  $(\hat{n}_{srt} - \hat{n}_{rt})$  held constant. But, this of course assumes I am able to estimate this parameter consistently.

There are a number of different challenges in identifying the parameter of interest  $\phi_4$  in equation (2.32). Firstly, the migrant share  $\lambda_{srt}$  is clearly endogenous to both wages and the productivity parameters ( $\hat{\alpha}_{srt}$  and  $\hat{z}_{srt}$ ), the latter of which are unobserved. These clearly affect migrants' location decisions. But,  $\lambda_{srt}$  is also endogenous to the relative labour supply,  $\hat{n}_{srt} - \hat{n}_{rt}$ .

A natural instrument for changes in the migrant share is the enclave instrument popularised by (Card, 2001). This instrument predicts the contribution of migration to local population growth, assuming that new migrants of each origin country settle in different areas proportionally according to the initial geographical distribution of their co-patriot communities. This is based on the premise that migrants have a strong propensity to cluster geographically, whether because of job networks or cultural amenities. Specifically, the instrument is constructed as follows:

$$IV_{rt} = \sum_o \frac{\phi_{rt-10}^o n_{ot}^{Mnew}}{n_{rt-10}} \quad (2.36)$$

where  $\phi_r^o$  is the share of migrants of origin  $o$  in year  $t - 10$  who are settled in city  $r$ ;  $n_{ot}^{Mnew}$  is the number of new migrants of origin  $o$  who enter the US between years  $t - 10$  and  $t$ ; and  $n_{rt-10}$  is the population of city  $r$  in year  $t - 10$ .

Suppose for the sake of argument that this instrument is uncorrelated with productivity growth. There will be concerns that the change in relative labour supply,  $\Delta \hat{n}_{srt} - \Delta \hat{n}_{rt}$ , is endogenous to this instrument. Unfortunately, there is no obvious second instrument which can separately identify the impact of changing labour supply on wages. However, empirically, it turns out that  $IV_{rt}$  has little effect on total labour supply within skill groups. The reason is that the new migrants displace natives within skill groups in local employment, largely through cross-city migration but also through higher native joblessness. This is a controversial claim, and I outline the evidence below. But, if this claim is taken as fact, a

consistent estimate of the effect of  $\lambda_{srt}$  can be attained by simply omitting  $\Delta\hat{n}_{srt} - \Delta\hat{n}_{rt}$  from the estimating equation.

As stated above, an important identifying assumption is that  $IV_{st}$  is uncorrelated with the productivity shocks,  $\hat{\alpha}_{srt}$  and  $\hat{z}_{srt}$ . This is commonly assumed in the empirical literature in this field. However, there are plausible reasons why this exclusion restriction might be violated. For example, if local productivity growth is persistent, historical migrant settlement patterns may not be exogenous to current productivity changes. Furthermore, Borjas et al. (1997) identify large cyclical components of local wage growth, across US census years, which are correlated with (but causally unrelated to) local immigration rates. One way to address this is to control for *predicted* employment and wage growth, based on initial industrial composition. Following the approach popularised by Bartik (1991), I weight employment growth (excluding the city under analysis), by two-digit industry, with initial city-level industry shares:

$$h_{rt}^n = \sum_i \phi_{rt-10}^i \frac{n_{i(-r)t} - n_{i(-r)t-10}}{n_{i(-r)t-10}}$$

where  $\phi_{rt-10}^i$  is the share of industry  $i$  in employment in city  $r$ , in year  $t - 10$ . And  $n_{i(-r)t}$  is national employment, excluding city  $r$ , in industry  $i$ . And following Beaudry et al. (2013), I construct an equivalent control for local wage growth:

$$h_{rt}^w = \sum_i \phi_{rt-10}^i \frac{w_{it} - w_{it-10}}{w_{it-10}}$$

where  $w_{it}$  is the mean national residualised wage<sup>16</sup> in industry  $i$  and year  $t$ .

Another empirical challenge is selection effects. In light of the large displacement of native workers, changes in composition of the local labour force may bias the estimates. In particular, if either high or low ability natives (within skill groups) are more likely to leave the city (on net) following an increase in the migrant share, this would introduce spurious composition effects which would be difficult to disentangle. I attempt to address this concern by comparing the wages of current and former residents in a city, exploiting data from the census on previous residence.

Finally, the two-type structure of the model (natives and migrants) is clearly a simplification. Over 70% of migrants in 2010 had been living in the country for more than 10 years, and over 40% for more than 20 years. There is no reason to believe these long-term migrants will have very different wage demands to natives: the wage assimilation literature attests to this. This creates some issues with measurement, since it is not clear which is the best indicator to prox the impact of migration on the reservation wage distribution: the migrant share alone is likely to be misleading. This is further complicated by the fact that the composition of migrants, in terms of years in the US, has changed dramatically since the acceleration of immigration after the 1960s. Furthermore, as noted above, there is good

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<sup>16</sup>Controlling for all available education fixed effects (indicating different years of schooling and years of college); interactions between all these education effects and a quartic in experience; a full set of 39 potential experience fixed effects; a gender dummy; race dummies (black, Hispanic).

reason to believe the migrant-native reservation wage gap is significantly smaller in lower skilled groups. Given these complexities, I take a flexible approach and estimate all specifications separately by skill group (identified by two-digit occupations) and census decade. Also, given the likely instability in the first stage, I focus entirely on reduced form estimates of all variables of interest on the enclave instrument.

With all the above in mind, I estimate the following empirical model:

$$\Delta x_{srt} = \beta_{st0} + \beta_{st1}IV_{rt} + \beta_{st2}h_{rt}^n + \beta_{st3}h_{rt}^w + \varepsilon_{srt} \quad (2.37)$$

where  $\Delta$  denotes a decadal change between census years, and  $\varepsilon_{srt}$  is the residual. I study reduce form effects of the instrument  $IV_{rt}$  on a range of different  $\Delta x_{srt}$  dependent variables; specifically: change in migrant share, growth in labour supply, growth in native wages, and growth in the native employment rate. I estimate these equations separately by two-digit occupation group and decade. And, in all specifications, I control for predicted growth in local employment  $h_{rt}^n$  and residual wages  $h_{rt}^w$ , based on initial industrial composition.

## 2.4 Data

### Census extracts and ACS samples

Most of my analysis is based on the IPUMS US census 2% extract of 1970, 5% extracts of 1980, 1990 and 2000, as well as the American Community Survey (ACS) 1% sample of 2010. I supplement the 2010 sample with ACS samples from 2008-9 and 2011-2 to boost the size of that cross-section. All this data has been organised by Ruggles et al. (2010a).

I identify local labour markets in the US with the Commuting Zones (CZs) originally developed by Tolbert and Sizer (1996). CZs were recently popularised by Autor and Dorn (2013) and Autor et al. (2013) as an alternative to metropolitan statistical areas (MSAs). MSAs cover only a limited proportion of the US landmass (unlike CZs whose coverage is universal); and there have been changes in geographical definitions over time: this would be particularly problematic for very long run analysis of this study. Tolbert and Sizler grouped the full set of US counties into 741 CZs, applying an algorithm to cross-county commuting data from the census of 1990. I restrict my analysis to the 722 CZs on the mainland. By in large, county boundaries have been very stable over time, and this makes long-run comparisons much more feasible. I make just one modification to the Tolbert-Sizler CZ scheme to ensure consistency: incorporating La Paz County (AZ) into the same CZ as Yuma County (AZ).<sup>17</sup>

The IPUMS micro-data does not identify specific counties, but each census year in my sample does include sub-state geographical identifiers. These identifiers vary across years<sup>18</sup>,

<sup>17</sup>Tolbert and Sizler allocate La Paz and Yuma to different CZs, but the two counties only separated in 1983: within the time-frame of our analysis.

<sup>18</sup>There are 405 county groups in the continental US in 1970, 1,148 county groups in 1980, 1,713 Public Use Microdata Areas (PUMAs) in 1990, 2,057 PUMAs in 2000 and the ACS until 2011, and 2,336 PUMAs in the ACS of 2012.

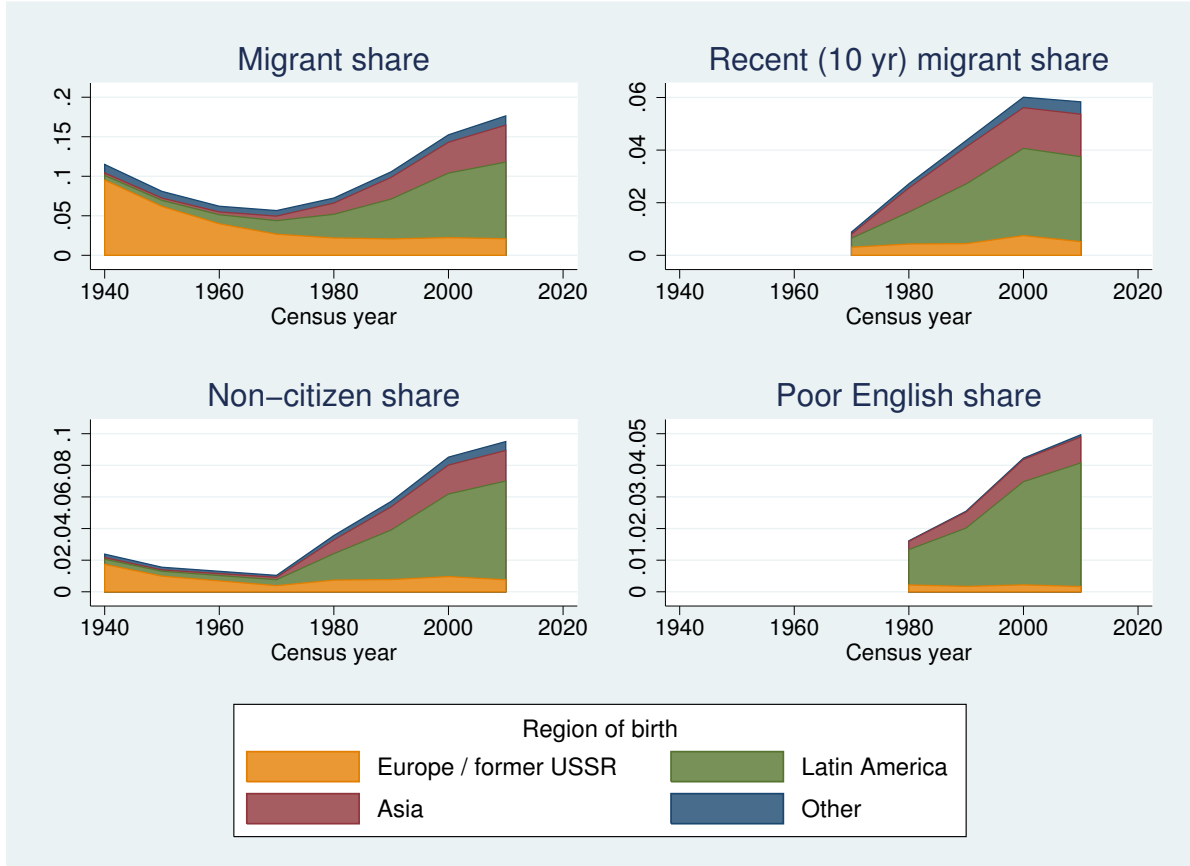


Figure 2.1: Share of migrants among 16-64s (Census/ACS 1940-2010)

and it is not possible to perfectly identify commuting zones based on their boundaries. Following Autor and Dorn (2013) and Autor et al. (2013), I estimate population allocations of the geographical identifiers into CZs<sup>19</sup>; and I impute CZ outcomes by appropriately weighting outcomes for the available geographical identifiers with these allocations.

My basic sample consists of all individuals aged between 16 and 64. When conducting analysis on wages, I restrict the sample to native-born employees outside the armed forces working full-time (at least 35 hours) and full-year (at least 40 weeks). These restrictions exclude workers with marginal attachment to the labour market: Borjas et al. (2008) argue these can confound estimates of unit labour prices. Wages are defined as weekly wages, calculated by dividing annual wage/salary income by weeks worked. Unless otherwise specified, all estimates based on CZ-level data are calculated using CZ weights corresponding to sample size.

<sup>19</sup>We take our data for the 1970 and 1980 population allocations from IPUMS: see <https://usa.ipums.org/usa/resources/volii/1970cgcc.xls> and <https://usa.ipums.org/usa/resources/volii/cg98stat.xls> respectively. For the remaining years, we generate the allocations using the MABLE/Geocorr applications on the Missouri Census Data Center website: <http://mcdc.missouri.edu>.



The first panel of Figure 2.1 shows the historical changes since 1940 in the migrant (that is, foreign-born) share of the US working-age population by region of origin. There has been a persistent decline in the share of workers born in Europe and the former USSR, as the old generation of migrants has died out. In contrast, since 1970, there has been a significant acceleration in migration from Latin America: based on the census, these workers now make up 10% of the labour force, compared to just 2% in 1970. Over the same period, there has also been a large growth in the stock of Asian migrants, who now contribute 5% of the labour force. Interestingly though, the migrant share has grown noticeably slower in the 2000s than the 1990s. These changes in migration flows can be better appreciated in the second panel, which plots the share of new migrants (who arrived in the previous ten years) as a share of the sample of 16-64s, since 1970 (arrival year is not available in earlier census extracts). This statistic grows steeply from under 1% in 1970 to 6% in 2000, with a small decline in the subsequent decade.

In terms of my model, it is questionable which statistic is the best proxy for the impact of migration on the reservation wage distribution. As noted above, there is substantial heterogeneity among migrants - with longer term migrants expected to be better integrated into local labour markets. As an alternative measure, the third panel plots the share of non-citizens over the full period. A better proxy for labour market integration is permanent residence status (that is, possession of a “green card”), which guarantees full access to the labour market - though this is not recorded in the census. Citizenship requires additional administrative hurdles, with the benefit of voting rights and security against deportation. In any case, there is a clear surge in the non-citizen share of 16-64s from 1% in 1970 to almost 10% in 2010. The fourth panel plots the population share who are migrants with poor English or none at all, though language proficiency is only reported since 1980. This statistic grows from 2% in 1980 to 5% in 2010.

An important concern is under-coverage of unauthorised migrants in the census - and unauthorised Mexicans in particular. Card and Lewis (2007) summarise some of the evidence, noting that the problem had eased considerably by the 2000 census. In particular, about 40% of unauthorised Mexicans were overlooked in the 1980 census (Borjas et al., 1991) and 30% in the 1990 census (Van Hook and Bean, 1998), but just 10% in 2000 (US Department of Homeland Security, 2003). Equivalently, 25% of all Mexican migrants were missed in 1980, 20% in 1990, and 6-8% in 2000. I argue below that this improvement in measurement should not affect the broad interpretation of my main empirical results.

## Defining labour markets by occupational skill

Much of the recent literature has studied the impact of immigration on native wages within broad education groups. But, in this study, I study variation across 2-digit occupations. This is for two reasons. Firstly, there is very large skill heterogeneity *within* these broad education groups, as I illustrate below. And secondly, using British evidence, Dustmann et al. (2013) show that migrants often downgrade in terms of skill on arrival. This may help explain why several studies (e.g. Card, 2009b; Manacorda et al., 2012; Ottaviano and Peri, 2012) find that natives and immigrants are weak substitutes within education groups. Dustmann et

al. study effects by percentile of the native wage distribution. But, if one believes migrants accept lower wages for similar work, occupations may be a better measure of skill.

I group workers into 74 minor occupation categories (excluding military jobs). I use the time-consistent 1990-based occupation classification provided by IPUMS (Ruggles et al., 2010a), although three of these occupations are not observed in 1970 and six in 1960. An important concern with using detailed job categories is that occupational mobility could complicate inference<sup>20</sup>. But, occupational mobility is of course limited by education level. Based on this premise, I rank occupations with a measure of skill imputed from the education of long-term residents' occupation group members; where "long-term residents" include everybody living in the US for more than 10 years. And, I then compare wage effects across the support of occupational skill.

To construct the skill measure, I calculate the mean wage of long-term residents within each detailed education category available in the 1990 cross-section<sup>21</sup>, restricting the sample to male long-term residents only. Using this data, I estimate the expected wage in each occupation group, conditional on the occupation's educational composition of long-term residents. For each occupation group, my skill measure is the percentile of the expected wage within the distribution of long-term residents. Notice that this measure of skill is consistent over time - to the extent that occupational skill requirements do not change; this is useful given that I am studying trends over many decades.

Using data from the ACS of 2010, Figure (2.2) plots the density of recent migrants (arriving in the last 10 years) along the support of occupational skill percentiles of long-term residents (i.e. natives or migrants in the US for longer than 10 years). A density of 1 along the full support would indicate that recent migrants and long-term residents share an identical skill distribution. This follows the method of Dustmann and Preston (2012), who present similar plots - though along the support of native wage percentiles rather than occupational skill. As it happens, the results look very similar.

Migrants are clearly concentrated at the bottom of the skill distribution and relatively weakly represented at the top. This is a significant result, because it seems inconsistent with the statistics on education - at least at the top of the distribution. On the one hand, recent migrants are indeed much more likely to be high school dropouts (under 12 years of schooling): these account for 27% of recent migrants, compared to just 8% of long-term residents. But, the share of college graduates (at least 4 years of college) is very similar: 29% of recent migrants, compared to 32% of long-term residents.

What explains this? The evidence shows that, within education groups, migrants tend to be lower skilled. This is consistent with British evidence from Dustmann et al. (2013) and Manacorda et al. (2012) on skill downgrading. Figure (2.3) plots the densities of long-term residents and recent migrants, by education group. Interestingly, the largest skill gaps fall in the middle of the education distribution: for high school graduates and those with some college education. This contrasts with the experience in the UK: Manacorda et al. only find

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<sup>20</sup>(Kambourov and Manovskii, 2009) report that, in the 1980s and 1990s, between 15% and 20% of workers in the PSID switched two-digit occupations each year.

<sup>21</sup>Specifically: nursery to grade 4; grades 5-8; grade 9; grade 10; grade 11; grade 12; 1 year of college; 2 years of college; 3 years of college; 4 years of college; 5+ years of college.

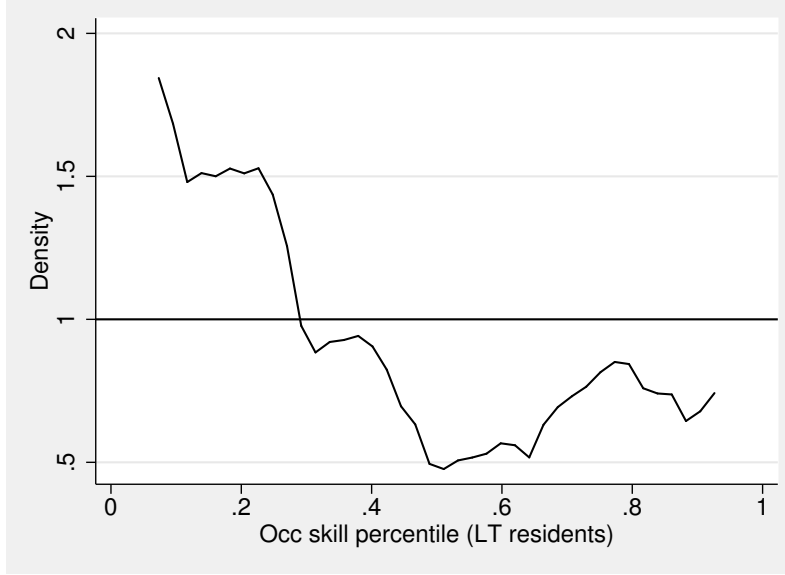


Figure 2.2: Position of recent migrants (last 10 years) in occupational skill distribution of long-term residents (ACS 2010)

evidence of migrant downgrading among migrant college graduates.

Another important point to take from Figure (2.3) is the very large skill heterogeneity *within* education groups. This suggests that a focus on education groups may be neglecting some important variation. In particular, there is a great deal of overlap between high school dropouts and high school graduates (though the latter are better represented in higher skilled jobs). This can perhaps help explain the instability of estimates of the substitutability between these two education groups in production, as reported by Borjas et al. (2012). The problem is that these groups are from from uniform.

## Enclave instrument

The left panel of Figure 2.4 plots  $IV_{rt}$ , the *predicted* 10-year migration rate (that is, the enclave instrument), by decade for a selection of cities. The right panel plots the *actual* 10-year migration rate, specifically  $\frac{n_{rt}^{Mnew}}{n_{rt-1}}$ ; where  $n_{rt}^{Mnew}$  is the number of migrants in city  $r$  and census year  $t$  who arrived in the previous ten years, and  $n_{rt-1}$  is the total local population in census year  $t - 10$ . Clearly, migration rates have grown hugely since the 1960s, though they have moderated somewhat in the last decade. By inspection, the instrument perform reasonably well as a predictor, with cities like Los Angeles, Miami and New York having consistently high rates of both predicted and actual migration after the 1960s.

Interestingly, migration rates in Miami are consistently underpredicted. This is because new Cuban migrants overwhelmingly locate in Miami (for example, two thirds of Cuban arrivals in the 1990s). But, the instrument excludes own-city new arrivals when predicting the local migration rate (see equation (2.36) above), so these Cubans largely do not register.

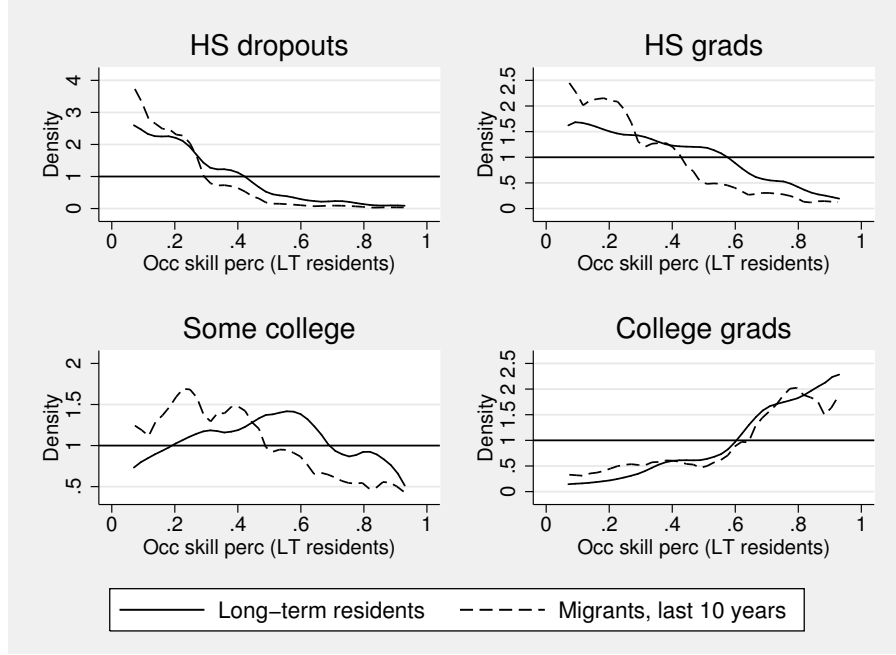


Figure 2.3: Position of recent migrants and long-term residents, by education, in skill distribution (ACS 2010)

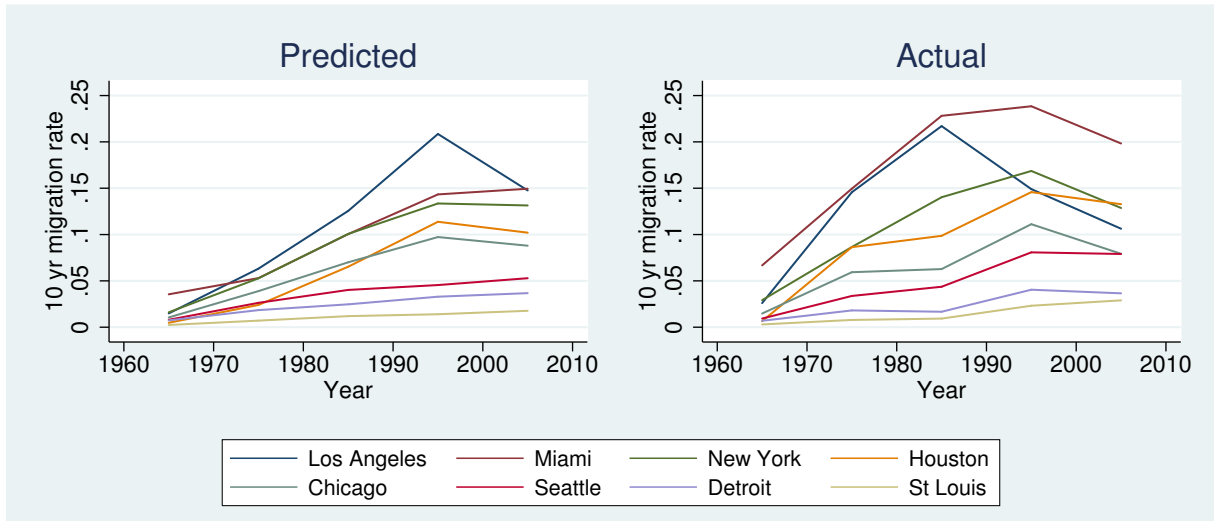


Figure 2.4: Predicted and actual local migration rates

Note: The left panel plots the predicted ten-year migration rate (i.e. the enclave instrument) for a selection of commuting zones, by decade: 1960-70, 1970-80, 1990-2000 and 2000-10. The right panel gives the actual migration rates in those same decades. The ten-year migration rate is estimated as the number of foreign-born individuals in some city  $r$  who arrived in the US in the previous ten years, as a proportion of the overall city  $r$  population ten years previously. The prediction is based on the premise that new migrants allocate across cities proportionally according to the initial geographical distribution of their co-patriots.; see equation (2.36) for the formula.

Notice also that migration to Los Angeles has declined markedly since its 1980s peak: from 22% to 11% in the 2000s. This is consistent with evidence on the changing geographical distribution of Mexican migrants reported in Card and Lewis (2007). Since this change was not driven by the *number* of Mexican migrants, the enclave instrument (in the left panel) lags the decline in the actual migration rate (in the right panel) for Los Angeles.

## 2.5 Empirical results

### Response of migration and migrant share

I begin by checking the predictive power of the enclave instrument. Specifically, I estimate equation (2.37) for each occupation and census decade, with the contribution of new migrants (arriving in the last ten years) to local occupation-specific employment growth as the dependent variable. That is,  $x_{srt} = \frac{n_{srt}^{Mnew}}{n_{srt-10}}$ , where  $n_{srt}^{Mnew}$  is the number of new migrants in occupation  $s$ , city  $r$  and census year  $t$ , who arrived in the US in the previous ten years; and  $n_{srt-10}$  is the total employment count in that city/occupation cell one decade previously. The  $\beta_{st1}$  estimates (the responses to the enclave instrument) are plotted, by census decade, in Figure 2.5. I have arranged these estimates along the support of occupational skill percentile, with marker sizes proportional to occupational employment counts.

Migration in the lower skill groups is much more responsive to the enclave instrument. This is for two reasons. Firstly, as illustrated in Figure (2.2), new migrants tend to be lower skilled than long-term residents. But, in results not reported here, I find that this does not account for the entire effect. This suggests that higher skilled migrants place less value on enclaves, perhaps because job match quality plays a relatively more important role in their location decision. Notice also that the migration response in the low skilled groups has declined somewhat with time. This is partly due to the reallocation of new Mexican migrants away from California (see Card and Lewis, 2007).

I repeat this exercise in Figure 2.6, but this time using the change in the occupation/city-specific migrant share as the dependent variable  $x_{srt}$ . This might be considered a first stage, to the extent that migrant share in my model determines the distribution of reservation wages and thus drives the wage outcomes. Having said that, as I mention above, migrant share is a somewhat flawed indicator of the reservation wage distribution - given the substantial heterogeneity within the migrant population.

The enclave instrument has a large impact on migrant share in low skilled jobs in the 1970s and 1980s, but a minimal effect in the 1990s and 2000s. This is despite the large impact in these latter decades on the migration rate in Figure 2.5. The explanation for this is large geographical displacement of longer-term migrants. I present evidence for geographical displacement below.

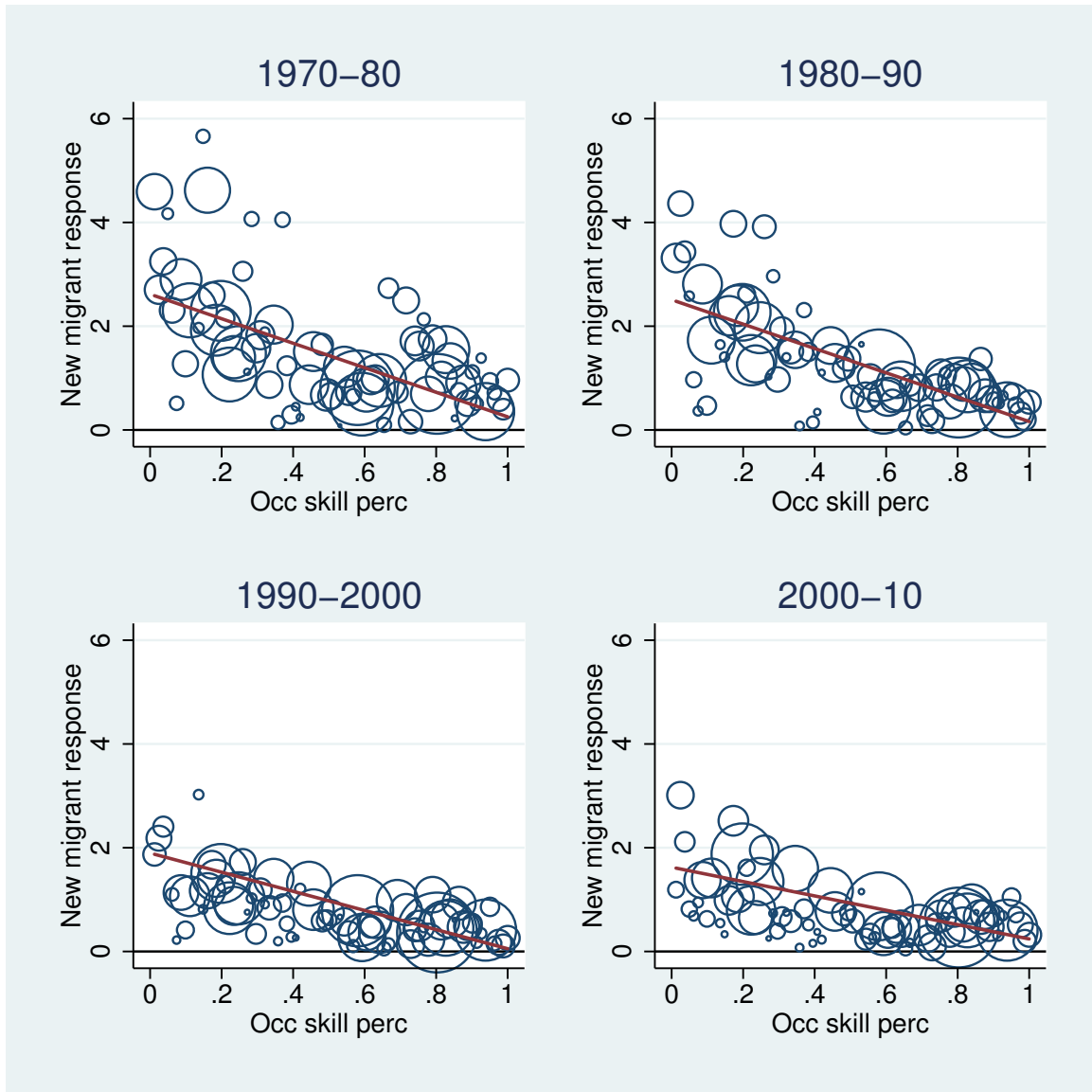


Figure 2.5: Occupation-specific effects of enclave IV on local migration rate

Note: For each panel in this figure (each corresponding to a particular decade), I estimate the effect of the city-level enclave instrument on the 10-year immigration rate separately for each occupation group. These regressions control for predicted employment and residual wage growth, as described above. Data points are weighted with national occupational counts. I also include an OLS fit line, which is estimated using national occupational counts as weights.

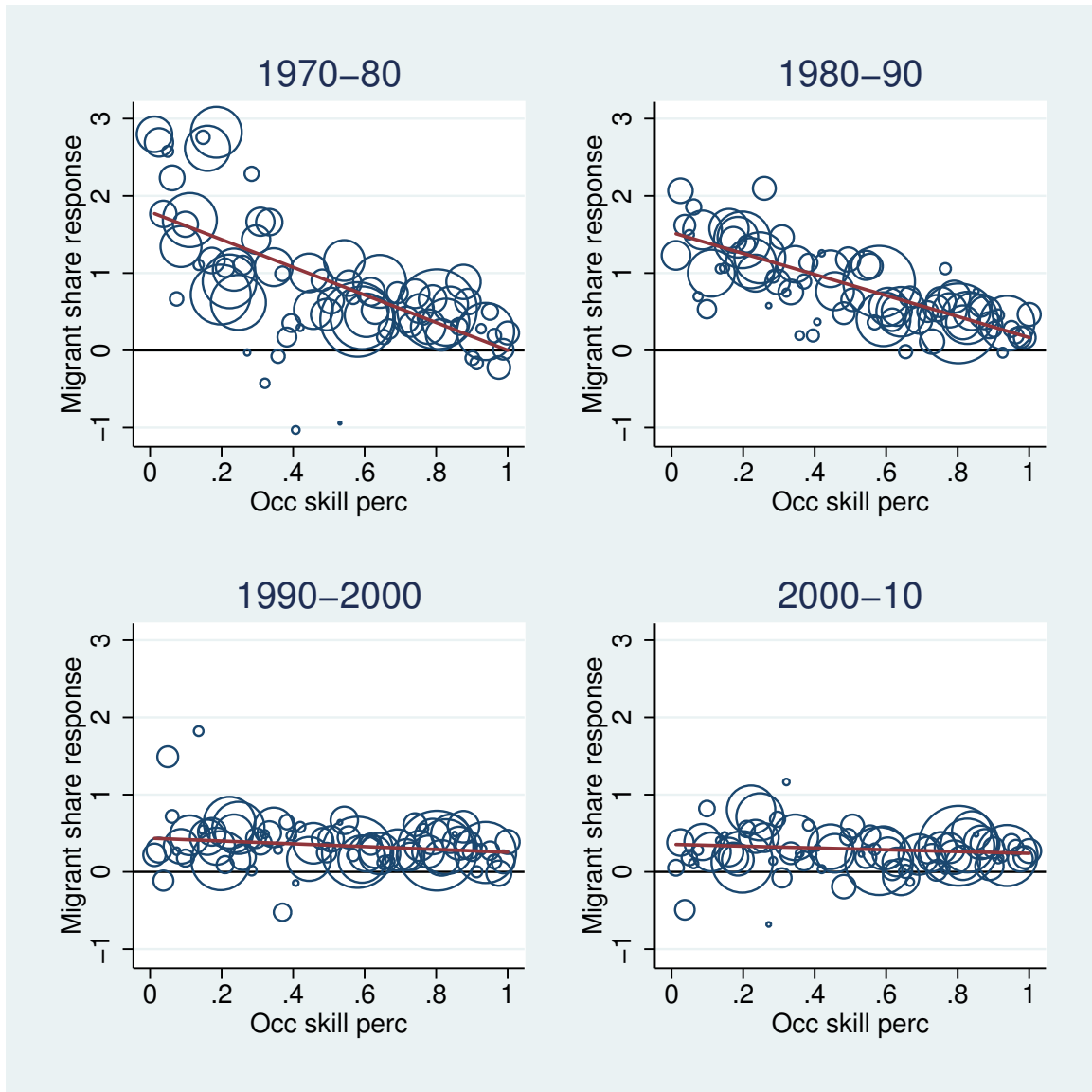


Figure 2.6: Occupation-specific effects of enclave IV on local migrant share

Note: For each panel in this figure (each corresponding to a particular decade), I estimate the effect of the city-level enclave instrument on the decadal change in migrant share separately for each occupation group. These regressions control for predicted employment and residual wage growth, as described above. Data points are weighted with national occupational counts. I also include an OLS fit line, which is estimated using national occupational counts as weights.

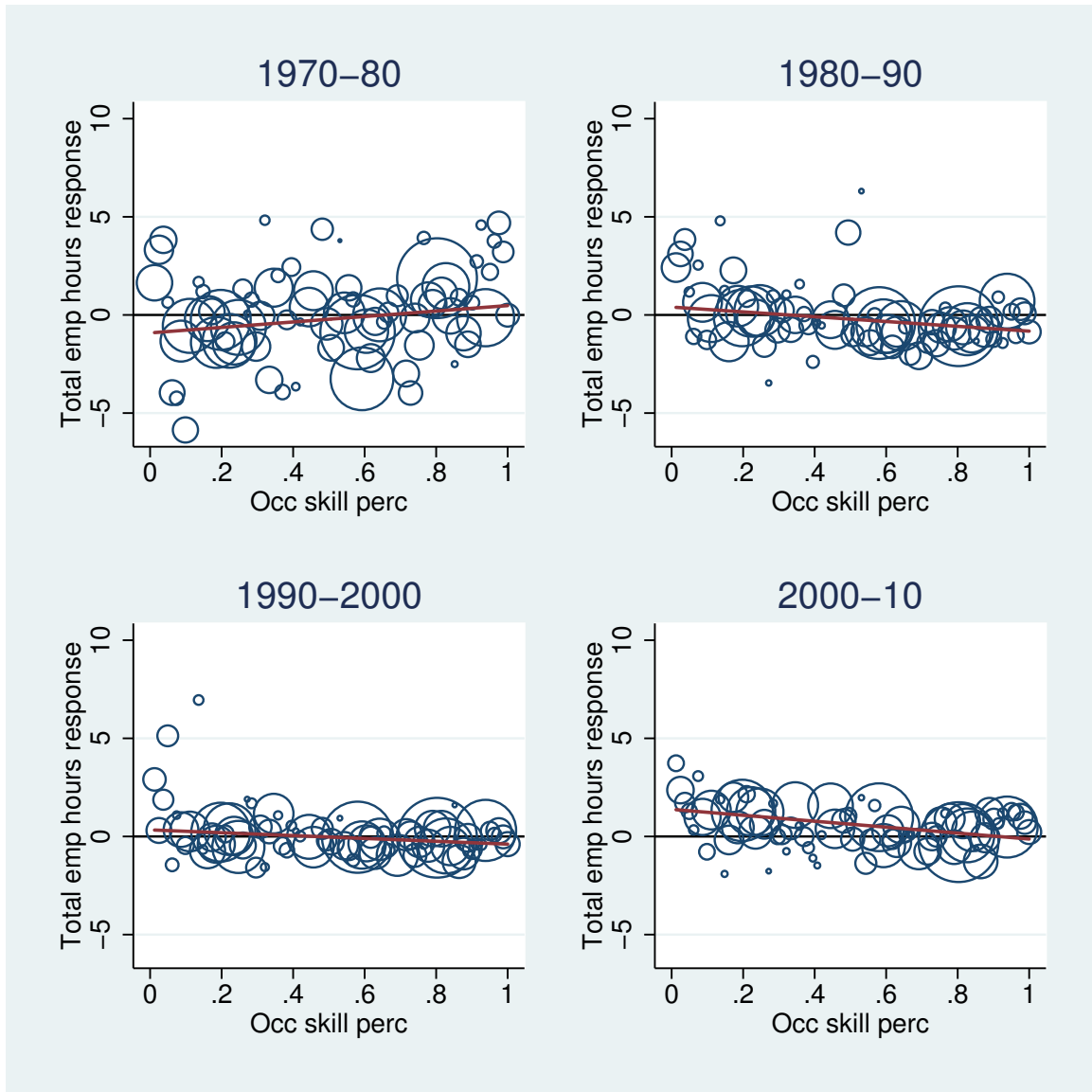


Figure 2.7: Occupation-specific effects of enclave IV on local employment hours

Note: For each panel in this figure (each corresponding to a particular decade), I estimate the effect of the city-level enclave instrument on the decadal change in log total employment hours separately for each occupation group. These regressions control for predicted employment and residual wage growth, as described above. Data points are weighted with national occupational counts. I also include an OLS fit line, which is estimated using national occupational counts as weights.



## Response of labour supply

As I argued above, my identification strategy is based on the assumption that the enclave instrument has little effect on overall employment within skill groups. And this claim is largely supported by Figure 2.7. Here, I plot the effect of the enclave instrument on decadal change in log total employment hours, within city/occupation cells. There appears in general to be little systematic effect on employment across skill groups. Assuming a model with constant returns to scale, this suggests there should be no substantial changes in marginal products.

An important exception is the 2000s, when there are positive effects on low skilled employment. The  $\beta_{st1}$  reduced form coefficients in the lowest skilled jobs in the 2000s are between 1 and 2 on average. Based on Figure 2.5, this is approximately the same as the effect on the migration rate in these jobs that decade. This suggests that the boost in migration is largely transferred to employment - suggesting there is little displacement overall.

However, there does appear to be large displacement in the previous decades. The most direct approach to analysing displacement is to study the cross-city migration decisions of individuals, rather than looking at changes in employment stocks over time. This is particularly useful in the context of this study, where mobility across occupation categories over time can conflate occupation-specific estimates. Furthermore, the results will be further conflated if the new migrants have children in the US: these will be classified as natives, though they are clearly driven by historical migration patterns.

I exploit information from the census cross-sections on respondents' city of residence five years previously. Unfortunately, the ACS (on which I base my results for the 2000s) only reports the city of residence one year previously, so the results are not comparable. Consequently, I restrict attention to the three previous decades.

Card (2001) also uses this data (specifically from the 1990 census) to study displacement. He disaggregates the workforce into groups according to time in the US. Following this approach, within occupations, cities and census years:

$$n_{srt} = n_{srt}^{Mnew} + n_{srt}^{LT} \quad (2.38)$$

where  $n_{srt}^{Mnew}$  is the employment count of new migrants in occupation  $s$ , city  $r$  and census year  $t$ , who arrived in the US in the last 5 years. And  $n_{srt}^{LT}$  is the number of longer-term residents, living in the US for more than 5 years. Then, taking differences and scaling by employment in occupation  $s$  and city  $r$  five years previously:

$$\frac{n_{srt} - n_{srt-5}}{n_{srt-5}} = \frac{n_{srt}^{Mnew}}{n_{srt-5}} + \frac{n_{srt}^{LT} - n_{srt-5}^{LT}}{n_{srt-5}} \quad (2.39)$$

where  $n_{srt-5}$  can be estimated from the cross-section of census year  $t$ , based on information on previous city of residence. This provides a disaggregation of the growth in employment (between periods  $t-5$  and  $t$ ) in city  $r$  of those currently employed in occupation  $s$ . I estimate equation (??), using both terms on the right hand side of equation (2.39) as dependent variables. I use the same enclave IV as before: the 10-year predicted migration rate.

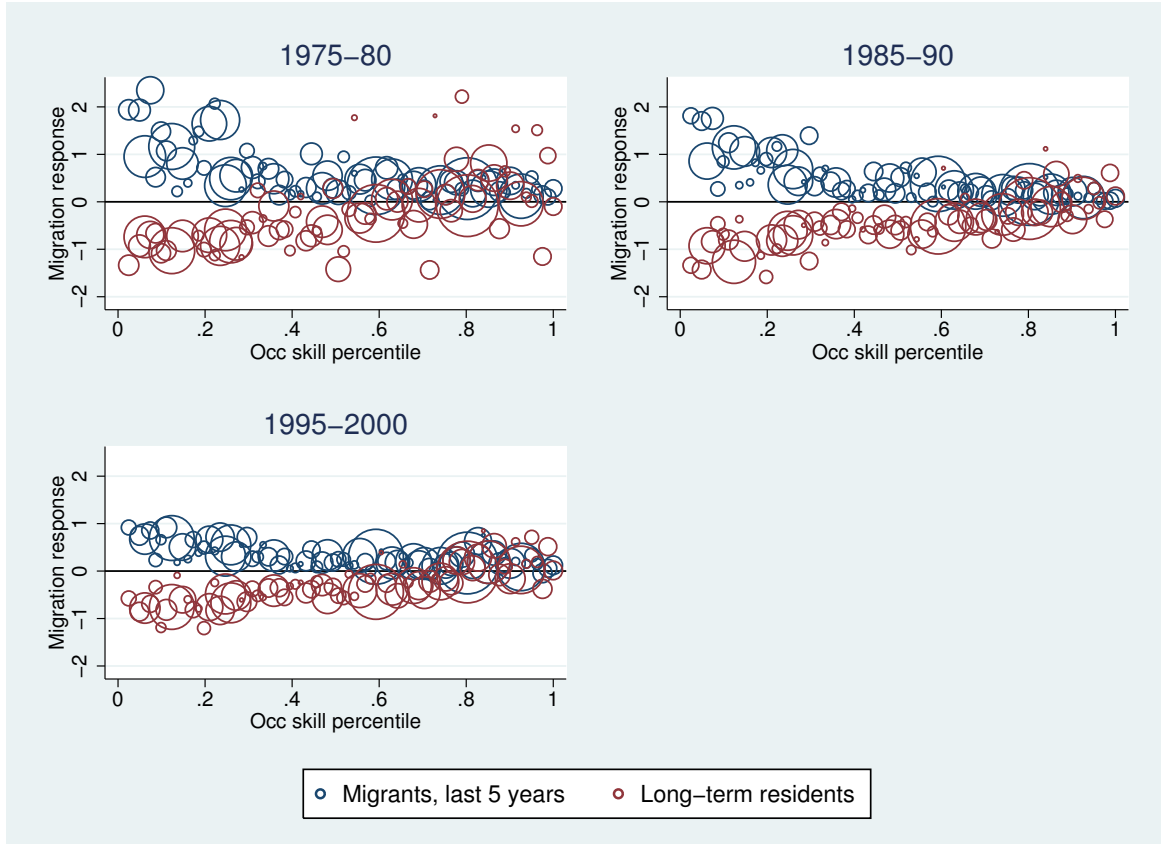


Figure 2.8: Decomposition of occupation-specific effects on 5-year local employment growth

Note: For each panel in this figure, I estimate the effects of the enclave IV on the 5-year growth of employment (in terms of number of workers) separately for each occupation group. I estimate these effects separately for migrants arriving in those five years (“new arrivals”) and all other workers (“long-term residents”). These regressions control for predicted employment and residual wage growth, as described above. Data points are weighted with national occupational counts. I also include an OLS fit line, which is estimated using national occupational counts as weights.

The results are presented in Figure 2.8, which reports  $\beta_{1st}$  estimates for the contribution to employment growth of new arrivals  $\frac{n_{gst}^{Mnew}}{n_{gst-5}}$  (in blue) and of past residents  $\frac{n_{srt}^{LT}-n_{srt-5}^{LT}}{n_{srt-5}}$  (in red). In each decade, the slopes of  $\beta_{1st}$  for each component are mirror images of one another. In particular, in the highest skilled occupations, the contribution of both components tends to be close to zero. But, in the lowest skilled jobs, there is a large positive contribution from new arrivals but a large *negative* contribution from long-term residents (i.e. large net out-migration) which appears to entirely offset the former effect. In results not reported here, I find that approximately half of the net outflows are due to longer-term migrants (in the US for more than five years) and about half are due to natives.

There are of course concerns over the coverage of unauthorised migrants in the census. But, this should not affect my conclusions on displacement. Specifically, the census coverage of unauthorised Mexicans has improved over time, especially in the 2000 census (see Section 2.4 above). Also, note that unauthorised Mexicans have tended to concentrate in low skilled occupations in popular migrant destination cities. As a result, the undercount of low skilled workers in these cities would have been more severe earlier in the sample period. So, the true low skilled employment growth in these cities may be *smaller* than estimated - suggesting that displacement may have been even larger.

Despite the fact that I use the same data and population decomposition as Card (2001), we come to opposite conclusions: he finds no geographical displacement. How can this be so? The answer lies in the delineation of skill groups and empirical specification. Card probabilistically assigns his sample into six occupation groups, based on their education and demographic characteristics (with the assignment based on predictions from a multinomial logit model). He then estimates the impact of migration on total population growth, controlling for occupation and city fixed effects. Specifically:

$$\frac{n_{srt} - n_{srt-5}}{n_{srt-5}} = \beta_0 + \beta_1 \frac{n_{srt}^{Mnew}}{n_{srt-5}} + d_s + d_r + \varepsilon_{srt} \quad (2.40)$$

where  $d_s$  and  $d_r$  are occupation and city fixed effects respectively; and  $\frac{n_{srt}^{Mnew}}{n_{srt-5}}$  is instrumented using an occupation-specific enclave instrument. Card estimates  $\beta_1$  to be approximately 1, which suggests there is no geographical displacement. The problem is that this specification is derived from a simple CES model which assumes that the substitutability between any two skill groups is identical. But, Card himself in later work (2009a) notes the fallibility of this assumption: he finds that high school graduates are perfect substitutes with high school dropouts, but imperfect substitutes with college graduates. Perfect substitutability is disputed by Borjas et al. (2012), but in any case, Figure (2.3) shows there is plenty of cross-over between the high-school dropout and high-school graduate occupational skill distributions. Indeed, when I replicate Card's methodology, but this time using just two skill groups (college graduates and non-graduates), I find that  $\beta_1$  is close to zero: this is consistent with my conclusions of very large displacement.

Several other studies in this literature are susceptible to similar criticisms. Card and DiNardo (2000) use a similar structure, though this time with three probabilistically assigned occupation groups rather than six. Card (2005) and Cortes (2008) divide the population into

two groups: high school dropouts and everybody else. They both find that a larger share of migrant dropouts in the local population has little impact on the overall dropout share; and they conclude that there is no displacement. But again, if high school dropouts and high school graduates are close substitutes in production, this specification will yield misleading results. For example, migrant dropouts are likely to be competing with native high school graduates.

Of course, mine is not the first to study to claim large geographical displacement. Borjas et al. (1997) find that the migrant contribution to state-level population growth is entirely offset by the (negative) native contribution. This result is dependent on taking differences over these population growth variables to purge the data of very long-run state-level growth in labour demand. Borjas (2006) finds that, for every ten migrants who enter a city, about six fewer natives choose to reside there. Interestingly, this level of displacement is not as strong as that suggested by Figure 2.8. But in any case, Peri and Sparber (2011) argue that Borjas' (2006) empirical model is misspecified, yielding a bias towards identifying migrant displacement of natives. On the other hand, Borjas also imposes restrictive assumptions on substitutability between skill groups which are likely to bias his results *against* displacement. Like Card (2001), his estimates are identified from skill differences within cities (this time using four skill groups), with high school dropouts and graduates assigned to distinct groups.

## Response of native wages

Given the large geographical displacement, I impute that marginal products have changed little - at least until the 2000s. Given this, I argue that any native wage effects can plausibly be understood to arise from the reservation wage channel. In Figure 2.9, I plot the responses of decadal change in log native wages  $\Delta \hat{w}_{srt}^N$  to the enclave instrument. In constructing  $\Delta \hat{w}_{gst}$ , I control for differences in worker composition across cities. Specifically, for each occupation group  $s$  and census year  $t$ , I regress log wages on a rich set of individual characteristics<sup>22</sup>. I estimate the city means of the residuals from these regressions, and I calculate  $\Delta \hat{w}_{gst}$  as the change in these city means between period census years  $t - 10$  and  $t$ .

In each decade, high skilled wages are unaffected by the enclave instrument, but low skilled wages always fall. The effect is small in the 1980s, 1990s and 2000s, but very large in the 1970s. To assess the magnitude of the effect, consider the experience of a popular migrant destination city like New York. As can be seen in Figure 2.4, the predicted migration rate (that is,  $IV_{rt}$ ) in New York in the 1970s was 5.3% (and the actual migration rate was 8.7%). And based on Figure (2.9), the reduced form wage elasticity coefficient for the lower skilled jobs is about -1.5 in the same decade. Assuming a causal effect has been successfully identified, this would suggest that immigration to New York in the 1970s triggered an 8 log point wage cut (for natives) in the lowest skilled jobs, through the reservation wage mechanism.<sup>23</sup>

<sup>22</sup>These include all available education fixed effects (indicating different years of schooling and years of college); interactions between all these education effects and a quartic in experience; a full set of 39 potential experience fixed effects; a gender dummy; race dummies (black, Hispanic).

<sup>23</sup>What is the overall national effect? A naive estimate can be computed by inputting the (population-

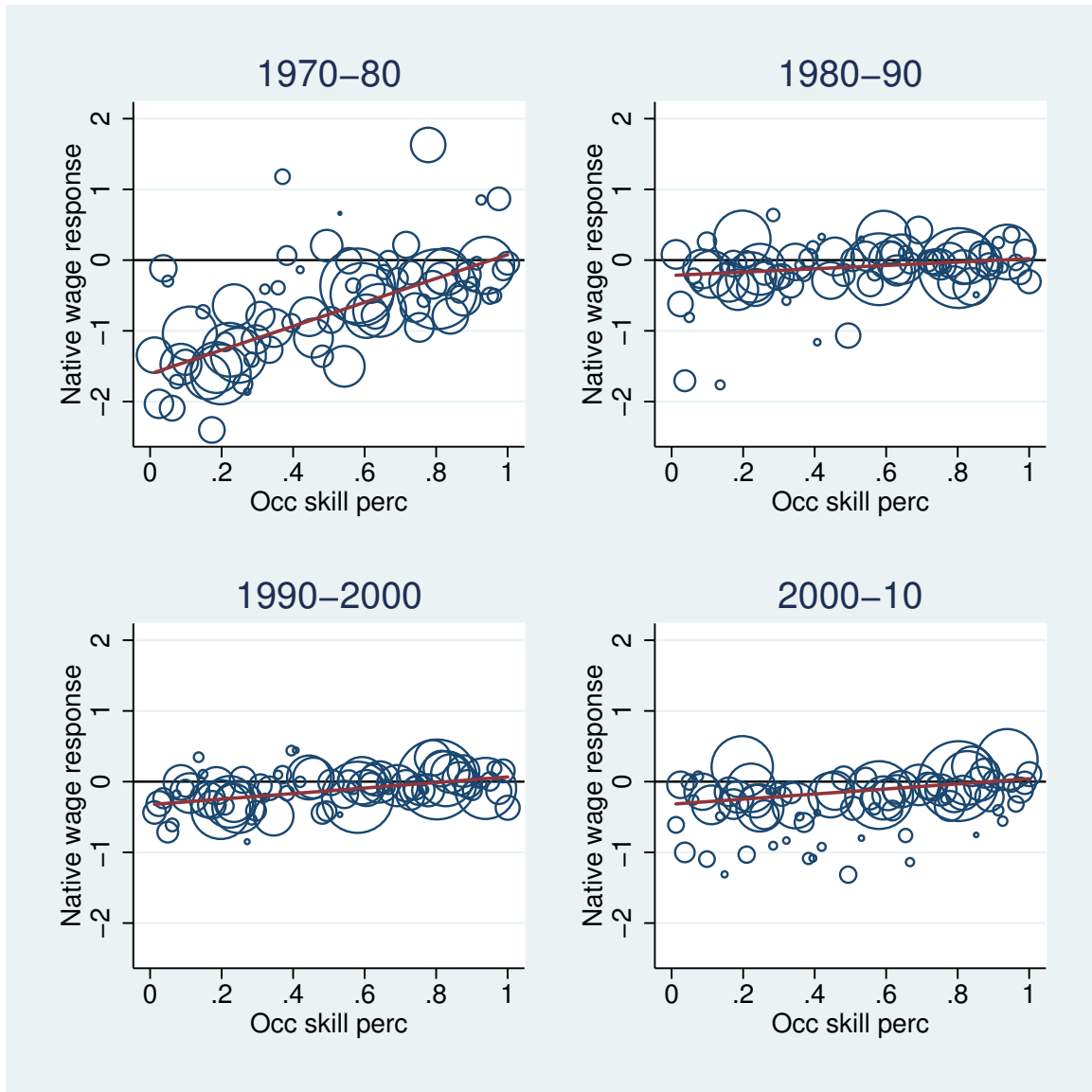


Figure 2.9: Occupation-specific effects of enclave IV on local native wages

Note: For each panel in this figure (each corresponding to a particular decade), I estimate the effect of the city-level enclave instrument on the decadal change in log residualised native wages separately for each occupation group. These regressions control for predicted employment and residual wage growth, as described above. Data points are weighted with national occupational counts. I also include an OLS fit line, which is estimated using national occupational counts as weights.

An alternative explanation for these wage effects, in light of the large geographical displacement identified above, is selection. Specifically, changes in city-level wages within occupation groups may merely reflect the sorting of workers of different unobserved abilities across cities (I already control for observed characteristics), rather than a local change in pay for a given individual. Indeed, Eeckhout et al. (2011) argue that sorting is the principle explanation for the relatively large earnings inequality currently found in larger US cities. In the context of migration, Bratsberg and Raaum (2012) argue that selective attrition of the lowest paid workers out of job categories subject to a large migrant influx can result in a spurious positive correlation, across job categories, between observed native wage changes and immigration rates.

To address this concern, I exploit information in the census on where respondents lived five years previously. In particular, I study the log differential between (1) the current average (residualised) wage of workers who were living in some city  $r$  *five years previously* and (2) the average wage of workers *currently* living in this city  $r$ . I carry out this exercise for the 1980 census data (thus, comparing residents of 1980 with residents of 1975), since it was the 1970s which saw the bulk of the wage effect.

In Figure 2.10, I estimate the response of this log wage differential in 1980 to the enclave instrument (predicting the migration rate of the 1970s), controlling as usual for predicted employment and residual wage growth. If selection is responsible for the wage results above, we should expect that, in cities with large migrant enclaves, the residualised wages of previous residents exceeds that of current residents - in low skilled jobs. However, the figure shows no evidence of such effects. This casts doubt on selection explanations for the wage effects in the 1970s.

It is instructive to compare my wage estimates with those in other studies which exploit cross-city variation in US census data for identification. Using data from the cross-section of 1990, Card (2001) finds that immigration to the most popular destination cities was responsible for a 3% larger native wage differential between the highest and lowest skilled occupations. But, this estimate is identified using the empirical model outlined in equation (2.40), which imposes restrictive assumptions on substitutability between skill groups in production. This can perhaps explain why his estimates are somewhat lower than mine. In contrast, when Borjas (2006) and Cortes (2008) study cross-city variation, they find statistically insignificant (though negative) effects of migration. This may be because their samples only begin in 1980, and their identification is based on decadal census differences. Consequently, they miss the large effects of the 1970s.

Cortes (2008) argues that her weak estimates are a consequence of imperfect substitutability between natives and migrants: she finds larger effects on the wages of longer-term migrants. But, Borjas (2006) explains the weak effects by the large geographical displacement of previous residents, which attenuates the city-level effect on marginal products. On

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weighted) average predicted migration rate (across all cities), equal to 2.1% in the 1970s. Given the reduced form wage coefficient of -1.5 in the lowest skilled jobs, this would imply a 3 log point wage cut. But, this is likely to be an overestimate: displacement of natives (to cities experiencing lower migration) is likely to have somewhat moderated the overall native wage impact. Of course, these estimates correspond to the reservation wage channel: they do not account for any changes in the marginal products of labour.

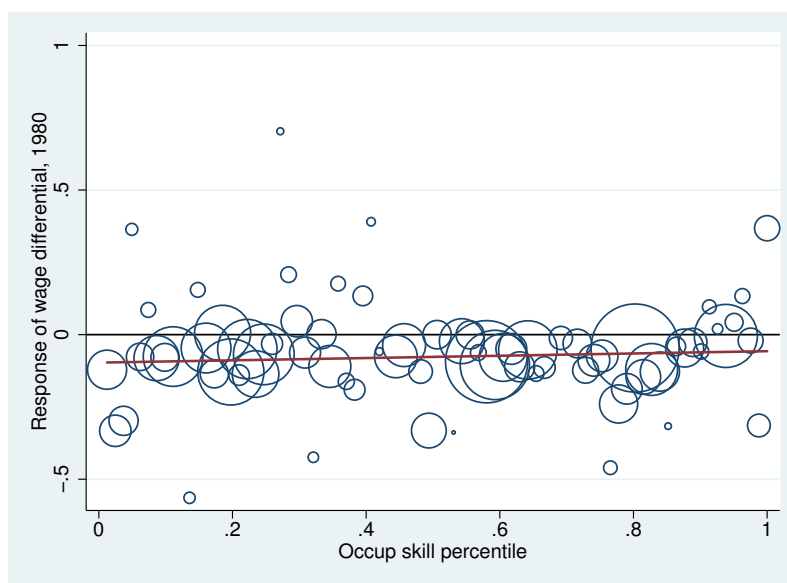


Figure 2.10: Occupation-specific effects of enclave IV on wage differential between current and previous residents

Note: The data points represent, for each occupation group, the response of the log wage differential in 1980, between current and previous residents, to the enclave IV. More specifically, the dependent variable of these regressions is the log difference between (1) the 1980 average (residualised) wage of workers living in some city  $r$  in 1975 and (2) the average wage of the 1980 residents of city  $r$ . These regressions control for predicted employment and residual wage growth, as described above. Data points are weighted with national occupational counts. I also include an OLS fit line, which is estimated using national occupational counts as weights.

the other hand, he estimates much larger wage effects using national-level data, identified from differences across time, education and experience groups. Of course, I exploit precisely this attenuation effect to argue my estimates are driven by the reservation wage channel. Based on the evidence of Figure 2.2, lower skilled workers are likely to have suffered a relative decline in marginal products as well. But, because of geographical displacement, these effects cannot be identified in cross-city estimates.

Another important point is the effect on *average* wages. Both Card (2001) and Borjas, 2006 identify effects using differences across skill groups (within cities), so average wage effects are not identified; and Cortes (2008) restricts attention to the low skilled. On the other hand, Card (2009a) finds large effects on average native wages in cross-city regressions (using the enclave instrument) - to the extent that local wages grows across the skill distribution, including for the lowest skilled natives. The difference in our results can be explained by my inclusion of the local wage and employment growth predictors, based on initial industrial composition. The problem is that the enclave instrument is strongly correlated with these predictors in the 1980s, which suggests Card's average wage estimates may be spurious.

## Response of native employment ratios

New migrants displace native employment geographically, as I have demonstrated above. But, they can also displace them into joblessness, if natives are unwilling to work at the new lower wages. In Figure 2.11, I plot the responses of decadal changes in the log native employment-population ratio to the enclave instrument, by census decade and occupation group.

Of course, the population denominator is unobserved because many jobless individuals do not report an occupation. I impute population in each occupation group by allocating non-employed workers probabilistically to different occupations, according to their city, age, gender, migrant status and education. I base these allocations on the distribution of those individuals who do report their occupation.

There is no systematic effect over the support of occupational skill in the 1970s. In the 1980s and 1990s, there is a large negative effect on low skilled employment ratios, with the high skilled unaffected. This pattern is also somewhat visible in the 2000s, though it is much weaker. What is the magnitude of these effects? Again, consider the example of New York, which had a predicted migration rate of 14% in the 1980s and 17% in the 1990s (see Figure 2.4). The reduced form coefficient in the lowest skill jobs, based on Figure 2.11, is about -0.7 in the 1980s and -0.6 in the 1990s. This suggests that immigration to New York triggered a 10 log point decline in native employment ratios among the lowest skilled workers, in each of these decades.

It is noteworthy that the low skilled migration shock in the 1970s is manifested in wages but not employment ratios; and the shock in the 1980s and 1990s is manifested in employment ratios, but with only a small effect on wages. One natural interpretation is that low skilled workers were willing to stomach the wage declines in the 1970s, but that was their limit. As the wage offer distribution continued to shift downwards in the 1980s and 1990s, native workers began to reject the lowest offers. This supported the level of recorded wages, but



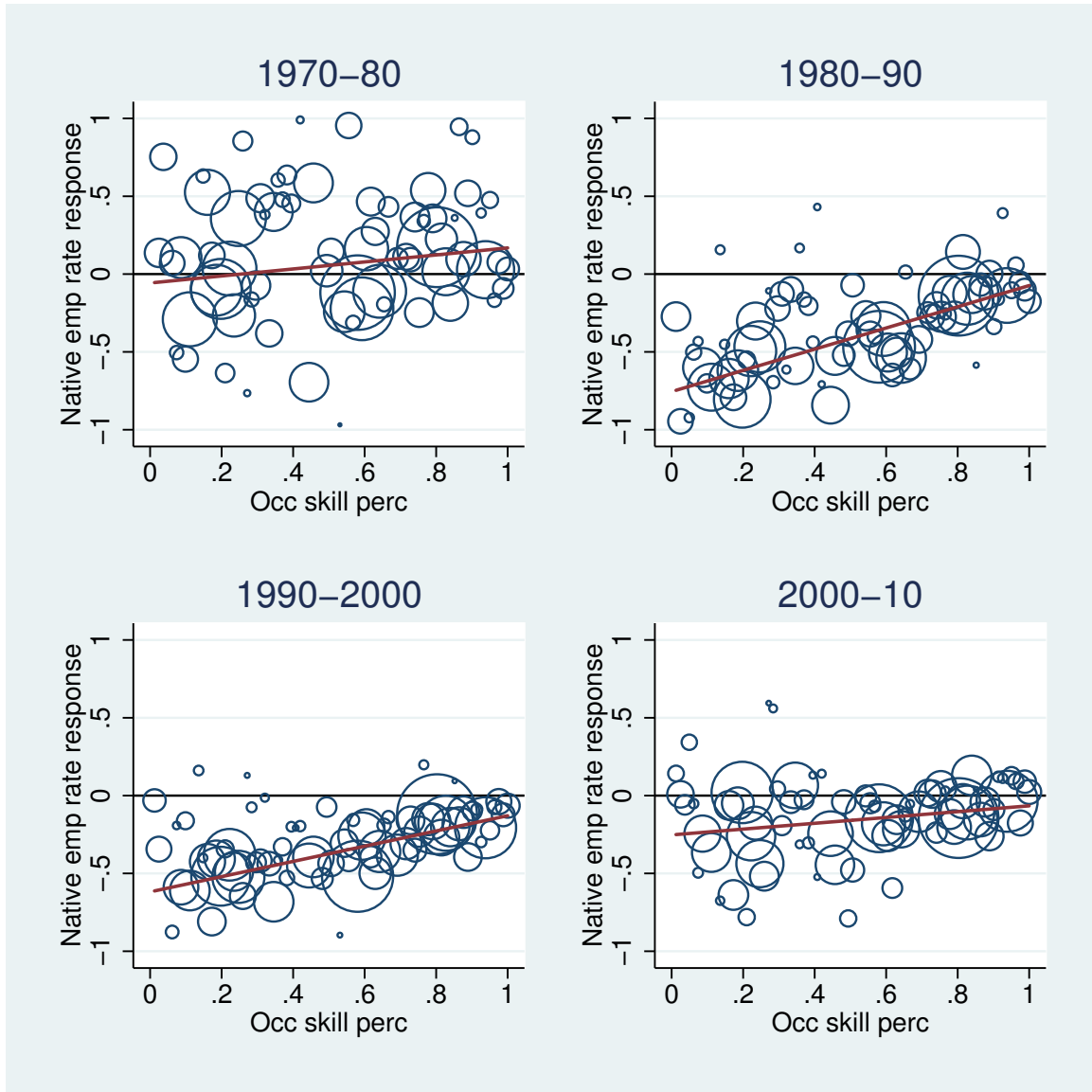


Figure 2.11: Occupation-specific effects of enclave IV on local native employment ratio

Note: For each panel in this figure (each corresponding to a particular decade), I estimate the effect of the city-level enclave instrument on the decadal change in the log native employment-population ratio, separately for each occupation group. Population in each occupation is estimated by probabilistically allocating the non-employed to different occupations, according to their observed characteristics (based on the distribution of employed workers across occupations). These regressions control for predicted employment and residual wage growth, as described above. Data points are weighted with national occupational counts. I also include an OLS fit line, which is estimated using national occupational counts as weights.

had adverse consequences for employment.

One important puzzle remains. Why were there large employment rate effects in the 1990s, even though the enclave instrument had little effect on local migrant share in that decade (see Figure 2.6 above)? One possible explanation is that the migrant share among *participants* was responsive to the enclave instrument in the 1990s, even if the migrant share among *residents* was not. In the model, I have treated migrant share as exogenous. But, of course, immigration to a particular city is a function of local firms' wage-setting decisions. Suppose firms, in cities with large *predicted* immigration, cut wages in response to a growing migrant share among market *participants*. This could explain *both* the lower native employment ratios *and* the lack of response of migrant share among residents (as new arrivals to the US chose to locate elsewhere).

## 2.6 Conclusion

In recent years, economic studies on the wages effects of immigration have focused on differences in *skill composition* between native and migrant workers. In a competitive model, these differences can affect the marginal products and wages of native workers at different skill levels. However, I argue that differences in *reservation wages* between migrants and natives (see e.g. Nanos and Schluter, 2013, for evidence) can also drive the wage effects. This is consistent with the popular view among politicians and the media that migrants are a source of “cheap labour” and *undercut* native wages.

I show how this can work in a model with wage-setting monopsonistic firms, where workers draw idiosyncratic employment utilities from random job matches. The model predicts that a rising share of migrants drives down the distribution of reservation wages, so firms cut wages for all workers (natives included) and native employment rates fall. I test these predictions empirically using cross-city data, within two-digit occupation groups. Geographical displacement of long-term residents by new migrants, within skill groups, allows me to identify effects coming through this channel: this is because marginal products are unlikely to have shifted much. And so, I interpret the native wage and employment rate effects as being driven by undercutting.

Throughout this study, I have neglected the wage effects caused by shifts in marginal products. But, using data on occupational skill distributions, I show that migrants are over-represented in low skilled jobs and under-represented at the top: basic theory suggests marginal products of low skilled labour should have fallen as a result. Previous work in the literature (such as Card, 2009b, and Ottaviano and Peri, 2012) has claimed otherwise; but it seems likely that their focus on broad education groups may conceal some important effects. However, I cannot identify these effects by exploiting cross-city variation, precisely because of the large geographical displacement - as has been noted by Borjas et al. (1997). This calls for new work on these shifts on marginal products, which goes beyond substitutability in production between broad education groups.

It is worth noting that, in the model I have described, the native welfare effects are not just limited to wages and employment probabilities. In the face of a migrant influx, firms

cut wage offers and this reduces natives' job rejection value. As a result, they are forced to accept lower wages or face unemployment. But, more broadly, they also accept jobs and locations with lower match utilities which are *unobserved* in the data. Therefore, limiting an analysis to native wages and employment may be justified in a competitive framework, but it risks neglecting potentially substantial costs associated with displacement across jobs and locations.

Beyond understanding the labour market effects of immigration, the model can also give important insights into policy. One seemingly paradoxical implication is that native wages can be better protected from a migrant influx if immigrants are *better integrated* into the labour market. If job search costs for migrants can be reduced, the reservation wage gap between migrants and natives will narrow - reducing the ability of firms to cut wage offers. This idea is not new: advocates of the US immigration reform bill of 2013, which eases the path to citizenship for unauthorised migrants, have argued that native workers are less likely to have their wages undercut by legal than illegal migrants (The White House, 2013). This calls for further research on the precise causes of reservation wage gaps to aid the development of appropriate policy responses.

# Chapter 3

## Skilled Mobility and the Job Surplus

### 3.1 Introduction

#### Contributions

It is well documented that higher skilled workers migrate more across cities: see, for example, Schwartz (1973) and Greenwood (1975). Recent work (Malamud and Wozniak, 2012; Machin et al., 2012<sup>1</sup>) has found that the effect of education on mobility is indeed causal. But, the mechanisms at play remain a source of vigorous debate. Bound and Holzer (2000) and Wozniak (2010) find that the low skilled are less likely to leave their city following a slump in local demand. Given this limited supply response, they argue that the low skilled face significant migration costs, whether due to credit constraints, lack of information or home attachment. This can help explain why they suffer greater wage volatility at the metropolitan level, as also documented by Topel (1986). And Moretti (2012) has argued forcefully that government should intervene through relocation vouchers (as part of unemployment insurance) to address this problem.

This focus on costs is natural, given the pervasiveness of the competitive urban framework proposed by Roback (1982). There, migration is treated as a form of spatial arbitrage between distinct local labour markets, with workers comparing local wages, amenity values and housing costs. Any sluggishness in this arbitrage process in low skilled markets must then be explained by large unspecified moving costs. But, in this chapter, I consider an alternative world where workers search for work in multiple cities simultaneously. And I argue the obstacles to low skilled mobility are precisely those “frictions” which explain the coexistence of unemployment and vacancies more generally. These frictions are larger in low skilled markets, because smaller job surpluses discourage search effort (on both sides of the market) and job creation.

The latter intuition was proposed by Wildasin (2000), in a discussion at the beginning of a study on the merits of local human capital investment (though the focus on vacancies is new

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<sup>1</sup>These studies exploit randomness from the Vietnam war draft in the US and a Norwegian compulsory schooling reform respectively.

to this chapter). He suggests the larger rents from skilled matches can be explained by skill specificity: the skills of better educated workers tend to be more job or task-specific. Given this, surgeons and university lecturers are likely to search in numerous cities for the ideal job, according to their specialisation; and recruiters will also spend more heavily on seeking out the ideal employee. In contrast, personal service workers have no such incentive: the jobs on offer are much the same wherever they go. In this sense, labour markets for higher skilled workers tend to be “better integrated” nationally. Further to Wildasin’s discussion, evidence from US panel data does indeed confirm that lower skilled workers are subject to smaller individual innovations in hourly wages<sup>2</sup> (Fitzgerald, 1999). And beyond skill specificity (or alternatively, the dispersion of match productivity), the relatively high *average* productivity of skilled labour clearly also inflates job surpluses.

I make two principle contributions in this chapter. Firstly, I present new evidence from the Current Population Survey (CPS) that the skill mobility gap is entirely driven by workers engaged in cross-city job search. I divide cross-county and cross-state migration into (1) “match-specific” moves, whose primary motivation relates to a match with a *specific* job; and (2) “non-match” moves, which are driven by other factors - whether family, housing, amenities or lack of jobs. It turns out that the better educated are much more likely to make a match-specific move in any given year; and the same is true of workers in higher skilled occupation groups *within* education groups. But, they are also significantly *less* likely to make a non-match move, of any particular type (family, housing, amenities, job search).

The fact that lower skilled workers are more likely to move city *in search* of work is of particular interest. Clearly, moving without a job in hand is a costly and risky strategy, which explains why it is relatively rare (just 4% of cross-county moves). Low skilled engagement in this strategy is testament to the relatively poor integration of national markets, which must serve as a large obstacle to migration.

My second contribution is to show how these qualitative results can arise from a multi-city job matching model. The model is very stylised, so I do not attempt to calibrate or estimate it. There are two frictions in the model: firstly, a fixed cost paid toward non-local matches; and secondly, uncertainty over an idiosyncratic match productivity parameter.<sup>3</sup> The probability that the match quality between a given worker and firm is revealed is increasing in the learning effort directed by each agent at their match partner. An important departure from Moen’s (1997) seminal study of directed search is that, here, workers and firms direct their search effort at match partners in multiple locations *simultaneously*. Labour markets are integrated geographically, with the extent of this integration determined by the parameters of the productivity process. In contrast, Moen restricts agents’ search activities to a *single* submarket: notice this is the identical criticism of Roback-style urban models that I made above.<sup>4</sup>

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<sup>2</sup>This finding is not well known, because much of the literature on earnings processes has focused on monthly or annual earnings, rather than hourly wages. I discuss this in further detail in Section 3.6 below.

<sup>3</sup>Unlike in the standard matching model, I assume the *existence* of all unemployed workers and vacancies is known. The unknown quantity is the match productivity between each worker-vacancy pair. See Manning (2003) for a discussion of these issues.

<sup>4</sup>Recent work by Beaudry et al. (2013) has integrated frictional job matching into traditional urban

There is some precedent for this alternative approach in the literature. Jackman and Savouri (1992) argue that internal migration should be understood as cross-city job matching. As evidence, they show that gross migration greatly exceeds net flows between UK regions; Wildasin (2000) makes a similar point using US data.<sup>5</sup> More recently, Manning and Petrongolo (2011) have investigated the geographical extent of labour markets, using a model where workers simultaneously make applications to jobs in multiple locations. Using geographically detailed data from the UK on local unemployment, vacancies and commuting patterns, they find that labour markets are very local: the utility of being offered a job declines exponentially at around 0.3km from a worker’s residence. However, they argue that higher skilled labour markets are likely to be broader. Marinescu and Rathelot (2013) estimate the model using job application data from the US. Also, following an earlier draft of this chapter, Lutgen and der Linden (2013) have developed a cross-city search model to explore the implications of more efficient online job search.

Rather than including multiple skill groups in the model, I assume workers are homogeneous and explore the impact of changing key parameters. To simulate the conditions faced by higher skilled workers, I study the implications of (1) raising average productivity and (2) increasing the dispersion of match productivity. In each case, the job surplus distribution shifts upwards (specifically, a hazard rate dominating transformation - though I do not yet have a proof for the match dispersion case). As a result, both workers and firms invest more heavily in learning effort; and assuming free entry, more jobs are created and market tightness grows. I validate these predictions with evidence on vacancy rates, application counts and recruitment expenditures. Also, larger job surpluses are more likely to dominate the non-local matching cost. And consequently, agents invest a larger share of their learning effort outside their home city, and most importantly, the rate of match-specific migration grows.

I then study the response to various local shocks. An adverse local productivity shock has relatively little effect on high skilled markets, because the shock is more easily absorbed by the large surpluses. In order to study the response to a given decline in local employment, I consider the extreme case where all jobs in a given city are destroyed. In that case, high skilled workers are more likely to match with jobs elsewhere, because the larger surpluses are more likely to dominate the migration cost. The low skilled suffer more in terms of welfare; and this can help explain why they are more likely to make a “non-match” move in search of work.

On the other hand, the migratory response to a shock to the local amenity value is larger among the low skilled. This is because the high skilled are less willing to give up their large job surpluses to realise gains in amenity value. This can help explain the negative skill

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frameworks, focusing on the joint determination of local wages, employment rates, house prices and city size. Each city is modelled with its own job matching function. When unemployed, workers choose a city and are then randomly matched with local firms. In contrast, in my model, agents are free to search *across* locations.

<sup>5</sup>The fact that gross flows exceed net flows is of course central to Wildasin’s contention that the movement of skilled workers is driven primarily by the specialisations of *individual* workers and firms: these idiosyncratic matches dominate aggregate adjustments between local areas.

gradient in non-match migration outlined above.

## Related Literature

Before moving on to the evidence and model, it is worth relating my work to other studies on the skill mobility gap. Estimating a dynamic structural model<sup>6</sup> on US panel data, Kennan (2013) also finds that college graduates face greater geographical wage dispersion and greater uncertainty in a location-match component in wages - compared to high skilled graduates. However, this can only explain a small fraction of the observed mobility gap: he puts the residual down to unspecified “costs”. But, this is not inconsistent with my findings or Wildasin’s (2000) claims: specifically, this residual is likely to be picking up differences in learning behaviour and market tightness across skill groups (these processes are absent from his model).

Other economists argue that the role of skill differentiated costs is minimal. Using a calibrated Roy model, Lkhagvasuren (2014) has found that differences in worker-location match productivity dispersion can account for most of the skill differences in gross migration rates; though his match dispersion parameter is itself calibrated (based on observed net migration rates across census divisions). But, like Kennan, he does not allow for endogenous learning effort or market tightness: he assumes perfect information on wage offers.

Notowidigdo (2011) argues that low skilled workers migrate less because they are relatively sheltered from local demand shocks. Specifically, they are better compensated by declining housing costs (they spend a larger share of their income on housing) and transfer payments. This is a strong claim, given Hoynes’ (2002) finding that the low skilled are subject to larger local wage and employment volatility. But importantly, it offers an alternative explanation for the relatively inelastic local supply of low skilled labour, aside from costly mobility (a principle aim of this chapter). On the other hand, he does not consider the role of match dispersion, which is likely to play a crucial role.

Many cost-based explanations have been proposed in the literature. Wozniak (2010) argues that the high skilled suffer less from credit constraints. Gregg et al. (2004) and Malamud and Wozniak (2012) suggest that college graduates have weaker home attachment, having already left home to study; and both articles also argue that long-distance job search is more costly for lower skilled workers.

My evidence of a negative skill gradient in non-match migration casts doubt on the importance of credit constraints and home attachment. But, this is not the case for skill-differentiated job search costs: clearly, these costs constrain only match-specific migration, and this is entirely consistent with the evidence reported in this chapter. But, I argue that these costs are just one (endogenous) manifestation of the underlying productivity processes, which are fundamentally responsible for the skill mobility gap. In this respect, an important contribution of my work is to identify the commonality between returns-based and cost-based explanations of the mobility gap. This distinction does matter for policy: it suggests

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<sup>6</sup>The model is based on Kennan and Walker (2011), who use it to measure the responsiveness of cross-state migration to expected gains in lifetime income, taking local wage processes as exogenous.

improvements in search technology (through the internet for example) cannot be a complete solution because they do not address the underlying cause.

As an aside, it is worth noting that one important practical component of “learning effort” and search costs lies in social networks. Social networks play an important role in job search: most studies tend to show around half of workers found their job through a personal contact (see Granovetter, 1995, for a survey and original analysis). Since higher skilled workers have more to gain from a successful match, my model suggests they should invest more heavily in developing broader social networks. Indeed, Granovetter finds that lower skilled workers are relatively more likely to have found their job through “family/social” contacts rather than “work” contacts (where the latter can be thought of as more “socially distant” than the former).

In the following section, I disaggregate the skill-mobility relationship in the CPS by reported reasons for moving. Section (3.3) describes the multi-city matching model. Then, in the following two sections, I assess the implications of average productivity and match dispersion for (1) market tightness, search behaviour and match-specific migration, and (2) for responses to local shocks. Section (3.6) presents evidence on skill differences in average productivity and match quality dispersion from the Survey of Income and Program Participation (SIPP), building on Fitzgerald’s (1999) findings on hourly wage volatility. And Section 3.7 reports some evidence on skill differences in market tightness and learning effort. I conclude in Section 3.8.

## 3.2 Migration patterns

### Basic facts

In this section, I study cross-county and cross-state migration patterns across education groups and by occupational skill *within* education groups. Migration across counties and states should be understood as long-distance moves, in the sense that most commuting happens within county or state boundaries: the American Community Survey of 2011 shows that 72% of employed Americans work in their county of residence and 96% in their state of residence.<sup>7</sup> For this exercise, I use the IPUMS CPS samples organised by King et al. (2010). The March CPS reports whether respondents moved county or state in the previous 12 months. And since 1999, respondents have also given their primary reason for moving. All statistics below are based on pooled cross-sections between 1999 and 2013.

My sample consists of labour force participants outside the armed forces aged 25 to 64. Restricting the sample to over-25s helps ensure my results are not conflated by individuals leaving college. In any case, I also exclude movers who explicitly report moving primarily to attend or leave college, as well as those who move because of natural disasters: these account for 2% of the remaining cross-county migrant sample. In Table 3.1, I report migration rates (1) across states and (2) across counties within states, broken down by primary reason for moving. The first column gives the percentage of the *full sample* who changed state for each

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<sup>7</sup>See Table S0801, American FactFinder (US Census Bureau).



recorded reason, and the second column reports the percentage of *cross-state migrants* who moved for each recorded reason. The final two columns repeat this exercise for cross-county moves within states. The CPS categorises primary motivations into a range of responses. I group these into three broad categories: (1) “match-specific” moves, where the move is driven by the requirements of a *specific* job; (2) “non-match” moves, which are motivated by factors unrelated to a specific job; and (3) “ambiguous” moves, which are difficult to categorise either way.

The bottom row shows that, each year, about 2% of the sample move across states and another 2% switch county within states. More than a third of cross-state moves are motivated by a specific job, compared with a quarter of within-state moves. Most of these match-specific moves relate to a job change or transfer, but many are driven by the desire for a shorter commute: in particular, this explains a third of within-state match-specific moves.

I group “non-match” moves into five subcategories: family, housing, amenities, looking for work and other reasons. Family and housing reasons each account for about a quarter of all moves across counties. Recorded family motivations include change in marital status and establishing own household, but the residual “other family reasons” category is the most common. Under housing motivations, the CPS records responses under desire for ownership, new/better housing and cheaper housing. The amenities category is significantly smaller, accounting for 6% of cross-state and within-state moves; most of these are driven by a desire for a “better neighbourhood”. Notice that I record “looking for work” as a non-match move, since it relates to local job availability rather than a *specific* job. This sort of speculative job search is notably rare, accounting for just 5% of cross-state and 3% of within-state moves.

Finally, I mark the CPS’s “other job-related reasons” category as “ambiguous” (in terms of whether they are match-specific), because it is not clear whether they are motivated by specific jobs or local job availability. This ambiguous group accounts for just 4% of cross-state and 2% of within-state moves.

Figure 3.1 plots annual cross-county migration rates disaggregated into three age groups: 25-34, 35-44 and 45-64. There is a clear education gradient, but only for the youngest experience group. Among the young, workers without college degrees (i.e. less than 4 years of college) have an average migration rate of 7.0%. But, the rate for young workers with a first college degree (4 years of college) is 9.0%; and for those with further degrees (5+ years), it is 10.7%. Among older workers, migration rates are lower (see Greenwood, 1975, and Kennan and Walker, 2011, for discussion of this point); and there is little difference across education groups.

Higher skilled workers also tend to move longer distances when they do migrate (Davis and Dingel, 2012). Figure 3.2 reports the share of all cross-county moves that are cross-state, again by education and age groups. For workers of all ages, there is a clear education gradient. On average, among high school dropouts, 41% of cross-county moves fall across states. But, this reaches 55% for workers with post-graduate qualifications. This effect can also be seen for very long distance moves: in Figure 3.3, I plot the share of cross-state moves that exceed 2000km<sup>8</sup>. Again, this share is increasing in education: from 16% among high

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<sup>8</sup>Migration distances are based on states’ internal latitude and longitude values, taken from the Census

Table 3.1: Breakdown of migration motivations in main sample

Primary reason	State moves		County moves (within states)	
	% full sample	% state migrants	% full sample	% county migrants
<hr/> MATCH-SPECIFIC <hr/>				
Specific job	0.71	37.42	0.49	24.71
New job or job transfer	0.65	34.20	0.32	16.25
Easier commute	0.06	3.23	0.17	8.46
<hr/> NON-MATCH <hr/>				
Family	0.47	24.64	0.53	26.76
Change in marital status	0.11	5.86	0.17	8.75
Establish own household	0.07	3.91	0.13	6.44
Other family reasons	0.28	14.86	0.23	11.57
Housing	0.36	19.31	0.69	34.74
Want to own home	0.08	4.08	0.19	9.31
New or better housing	0.12	6.35	0.24	12.16
Cheaper housing	0.07	3.48	0.11	5.30
Other housing reasons	0.10	5.39	0.16	7.97
Amenities	0.12	5.94	0.13	5.54
Better neighbourhood	0.05	2.46	0.08	4.23
Climate, health, retirement	0.07	3.48	0.03	1.30
Looking for work	0.10	5.43	0.05	2.69
Other reasons	0.06	3.10	0.06	3.08
<hr/> AMBIGUOUS <hr/>				
Other job-related reasons	0.08	4.16	0.05	2.48
ALL REASONS	1.89	100	2.00	100

This table presents migration rates by primary reason in CPS cross-sections between 1999 and 2013, excluding those who move primarily to attend or leave or college or because of natural disasters (these account for 2

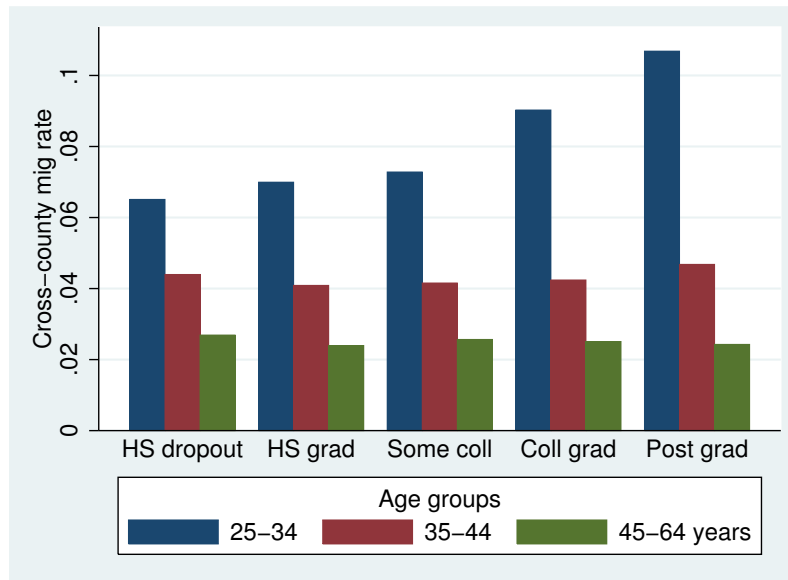


Figure 3.1: Annual migration rates by education and experience

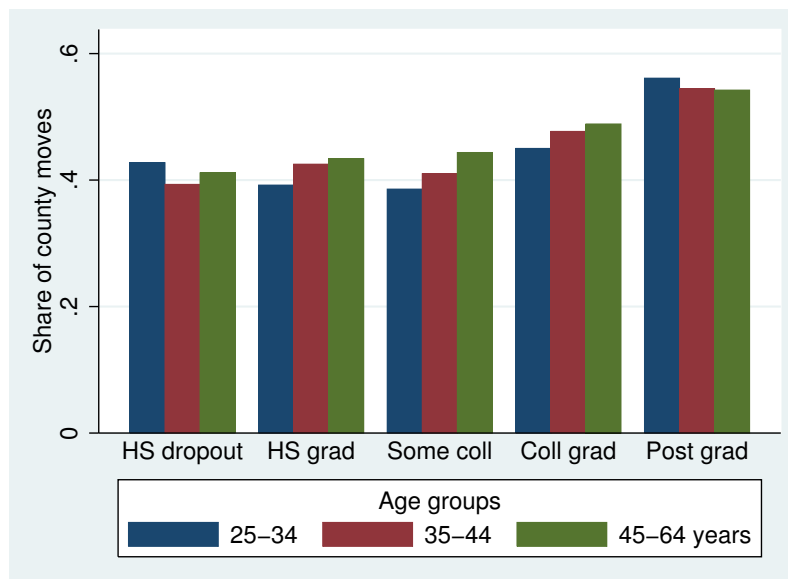


Figure 3.2: Share of cross-county moves which are cross-state

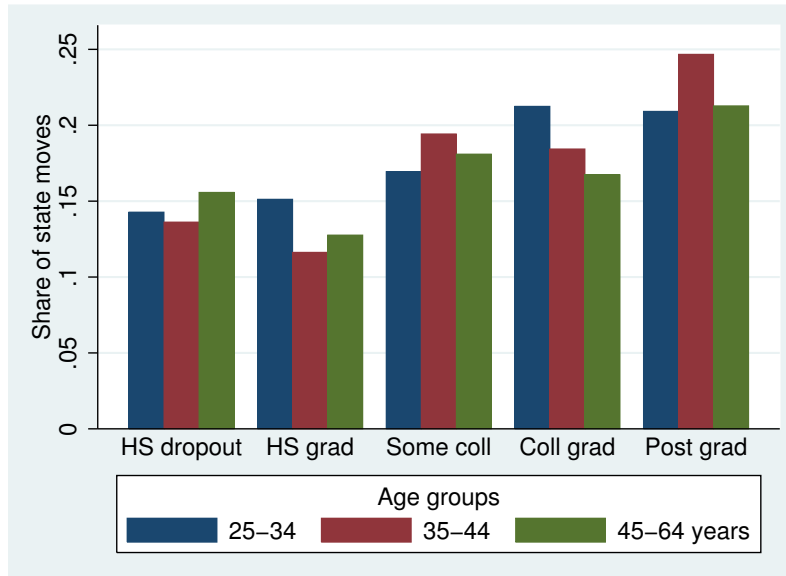


Figure 3.3: Share of cross-state moves exceeding 2000km

school dropouts to 23% among post-graduates.

The key part of this analysis is the disaggregation of these migration patterns by reasons for moving. Figure 3.4 reports separately the annual rate of match-specific and non-match migration across counties. The difference between the two is striking. There is a very steep and positive education gradient in match-specific rates. The effect is strongest among the youngest workers: 1.2% of high-school dropouts make match-specific moves each year, compared with 5.8% of post-graduates; but even among older workers, there are positive effects. However, for non-match moves, there is a *negative* education gradient. For workers under 35, this gradient appears weak and only manifested for post-graduates. For 35-44s though, the non-match migration rate falls from 3.4% for high-school dropouts to 2.2% for post-graduates; and there is a clear but shallower education slope for the oldest age group.

These migration rates appear to be small, so the differences between education groups might be considered inconsequential. But, in the context of job-matching, these observed rates should really be compared with job finding rates. In my CPS sample, only 12% of workers have a different job to 12 months ago<sup>9</sup>. As Figure 3.5 shows, job changing rates do increase somewhat with education. But, the effect is relatively small: among the youngest age group, growing from 14% for high school dropouts to 19% for postgraduates. As a result, these differences cannot explain the skill gap in migration: among 25-34s who *changed job* in the previous 12 months, 2.4% of high-school dropouts made a match-specific cross-county move, compared with 16% of post-graduates. This is a very large difference.

Bureau: [http://www.census.gov/geo/www/2010census/statearea\\_intpt.html](http://www.census.gov/geo/www/2010census/statearea_intpt.html). I calculate the distances using the GEODIST Stata module, written by (Picard, 2012).

<sup>9</sup>I define a worker as “changing job” in the CPS if (1) he worked for two or more employers over the past year or (2) he was unemployed or in education for part of the previous year.

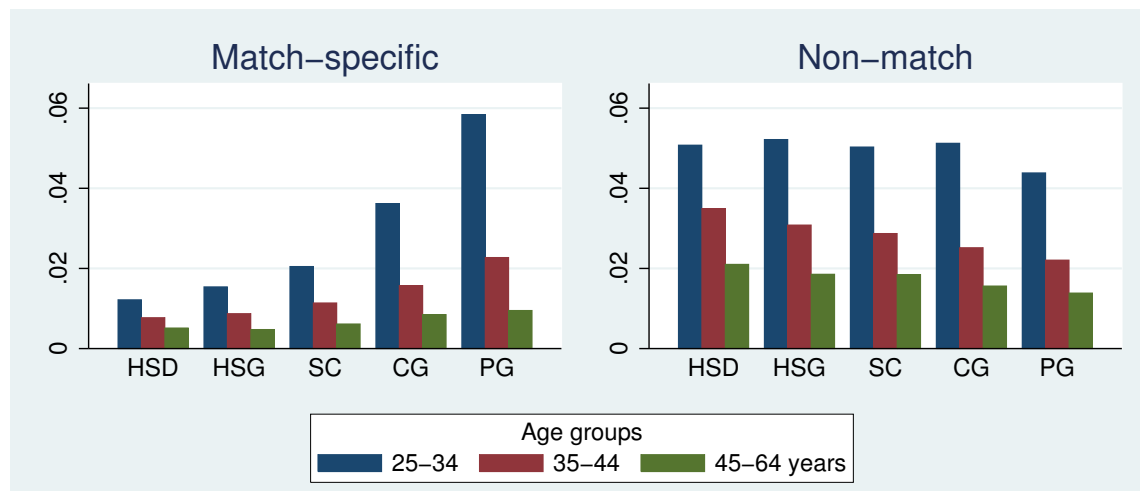


Figure 3.4: Annual migration rates disaggregated by reported reason

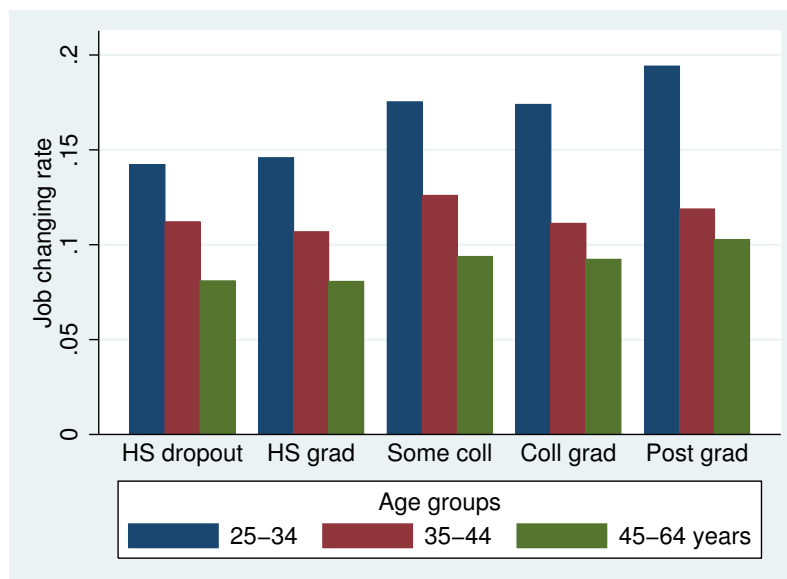


Figure 3.5: Share of workers changing job each year

Table 3.2: Marginal effects from probit regressions on match-specific migration incidence

*Dependent variable: takes 1 for a cross-county match-specific move in previous 12 months*

Age group	25-34 (1)	25-34 (2)	35-44 (3)	35-44 (4)	45-64 (5)	45-64 (6)
High-school graduate	0.381** (0.176)	0.200 (0.177)	0.104 (0.113)	0.012 (0.115)	-0.016 (0.065)	-0.051 (0.066)
Some college	1.054*** (0.176)	0.675*** (0.181)	0.455*** (0.113)	0.261** (0.117)	0.155** (0.065)	0.083 (0.068)
College graduate	2.279*** (0.177)	1.610*** (0.191)	0.824*** (0.114)	0.506*** (0.124)	0.390*** (0.066)	0.277*** (0.072)
Post-graduate	3.786*** (0.192)	2.933*** (0.211)	1.310*** (0.120)	0.904*** (0.134)	0.482*** (0.069)	0.338*** (0.077)
Occupational skill last year (ranges from 0 to 1)		1.585*** (0.178)		0.773*** (0.114)		0.276*** (0.00067)
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	315,303	315,303	384,840	384,840	521,642	521,642
Migration rate (%)	2.542	2.542	1.243	1.243	0.656	0.656

Each column reports marginal effects of various education levels from probit regressions on annual cross-county match-specific migration incidence. I also estimate models which control for occupational skill percentile measure, as described in footnote 12. I report results separately for three age group samples. For binary education indicators, marginal effects relate to change in regressor from 0 to 1 (the excluded category is high-school dropout); for the occupational skill measure, I report the average marginal effect. All marginal effects scaled by 100 and reported in percentage terms. Estimates are based on a panel of CPS cross-sections between 1999 and 2013. The sample is restricted to labour force participants outside the armed forces. Individuals who move primarily to attend or leave or college or because of natural disasters are also excluded (these account for 2

Table 3.3: Marginal effects from probit regressions on non-match migration incidence

*Dependent variable: takes 1 for a cross-county non-match move in previous 12 months*

Age group	25-34 (1)	25-34 (2)	35-44 (3)	35-44 (4)	45-64 (5)	45-64 (6)
High-school graduate	-0.482** (0.195)	-0.384* (0.197)	-0.441*** (0.136)	-0.388*** (0.137)	-0.170* (0.092)	-0.148 (0.093)
Some college	-0.772*** (0.2)	-0.561*** (0.207)	-0.588*** (0.141)	-0.474*** (0.146)	-0.169* (0.094)	-0.124 (0.099)
College graduate	-0.889*** (0.209)	-0.508** (0.230)	-0.849*** (0.149)	-0.660*** (0.163)	-0.343*** (0.099)	-0.271** (0.110)
Post-graduate	-1.209*** (0.252)	-0.718*** (0.280)	-1.101*** (0.169)	-0.857*** (0.189)	-0.518*** (0.107)	-0.426*** (0.123)
Occupational skill last year (ranges from 0 to 1)		-0.929*** (0.230)		-0.463*** (0.161)		-0.179* (0.106)
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	315,303	315,303	384,840	384,840	521,642	521,642
Migration rate (%)	5.053	5.053	2.847	2.847	1.753	1.753

Each column reports marginal effects of various education levels from probit regressions on annual cross-county non-match migration incidence. I also estimate models which control for occupational skill percentile measure, as described in footnote 12. I report results separately for three age group samples. For binary education indicators, marginal effects relate to change in regressor from 0 to 1 (the excluded category is high-school dropout); for the occupational skill measure, I report the average marginal effect. All marginal effects scaled by 100 and reported in percentage terms. Estimates are based on a panel of CPS cross-sections between 1999 and 2013. The sample is restricted to labour force participants outside the armed forces. Individuals who move primarily to attend or leave or college or because of natural disasters are also excluded (these account for 2

In Tables 3.2 and 3.3, I show that the migration patterns described above are robust to observable characteristics for match-specific and non-match moves respectively; and I also show that migration rates are responsive to occupational skill *within* education groups. The reported coefficients on education indicators (high-school graduate, some college, college graduate and post-graduate) are marginal effects (expressed in percentage point terms) from individual-level probit regressions on binary migration variables. The omitted category is high-school dropout. I also estimate models controlling for an occupational skill percentile measure as a regressor, imputed from the educational composition of the occupation group, which varies from 0 (for the lowest skilled jobs) to 1 (for the highest)<sup>10</sup>. I base this measure on workers' occupation 12 months prior to the survey<sup>11</sup> (which the CPS reports) to reduce concerns about endogeneity. In each regression, I also control for a detailed range of individual characteristics.<sup>12</sup>

For match-specific cross-county migration in Table 3.2, there are positive and strongly significant education effects within each age group. The magnitude of the effects are very similar to the unconditional effects reported in Figure 3.4, with the effects decreasing in age. Columns 2, 4 and 6 show that job-motivated migration rates are also increasing in occupational skill *within* education groups. The effect is large: moving from the bottom to the top occupational skill level has a comparable effect to moving from high school dropout to college graduate. Interestingly also, occupational skill explains about a third of the observed effect from education levels; this can be appreciated by comparing the education coefficients across adjacent columns. These within-education effects are important, because they suggest the migration patterns are driven somehow by the different *jobs* that workers do, rather than just by their educational qualifications.

Conversely, for non-match moves in Table 3.2, the education slope is negative and strongly significant in each age category - including for the under 35s, for whom the slope was weak in Figure 3.4. Results from some experimentation (not reported here) reveal that the difference comes from controlling for a Hispanic indicator in the Table 3.2 regressions: Hispanics tend to make relatively few non-match moves, despite having relatively low education levels. For the older two age groups though, the size of the effects is similar to the unconditional statistics in Figure 3.4. Columns 2, 4 and 6 show that non-match migration rates are significantly decreasing in occupational skill level *within* education groups. The effect of job skill is again substantial: the effect of moving from the lowest to highest skill category is comparable to

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<sup>10</sup>More specifically, I calculate the mean wage within each of 16 detailed education category available in the CPS. Using this data, I estimate the expected wage in each occupation group, conditional on the occupation's educational composition. For each occupation group, my skill measure is the percentile of the expected wage within the distribution of occupations. I use the time-consistent 1990-based classification provided by IPUMS, grouping workers into 74 minor occupation categories.

<sup>11</sup>Of course, I have no data for workers who had no job 12 months prior to the survey, who account for 3% of the sample. For these observations, I impute an expected occupational percentile based on a linear regression of occupational percentile (for the sample who had a job in the previous year) on the 16 available education indicators, as well as detailed demographic characteristics (see following footnote).

<sup>12</sup>Specifically: age, age squared, black and Hispanic race dummies, immigrant status, marital status, a range of indicators for number of own children, a gender indicator which is also interacted with all previously mentioned variables, and a set of year fixed effects (for the individual CPS cross-sections).



the effect of moving from high school dropout to postgraduate education. And similarly to non-match migration, occupational skill explains about a third of the observed education effects (comparing adjacent columns).

## Further detail and robustness

Of course, the non-match gradients reported above are aggregates over *many* distinct motivations. But, I show below that the negative slopes are common across more disaggregated non-match categories, related to family, housing and amenities. I report the results from this disaggregation exercise in Table 3.4. Firstly, I decompose cross-county moves into cross-state (first four rows) and within-state cross-county moves (last four rows). Secondly, across the rows of the table, I decompose match-specific and non-match moves into more detailed reasons, and I also include results for the “ambiguous” category. For simplicity though, I pool all age groups together - so the education coefficients represent average effects across the full age sample.

The first row reports effects for all motivations combined. Interestingly, the positive education gradient is only present for cross-state moves and not within-state. This is for two reasons. Firstly, the education slope for migration motivated by a new job (the following row) is much larger for cross-state than within-state moves (though positive and significant for both): controlling for individual characteristics, post-graduates are on average 0.97 percentage points more likely than high school dropouts to make a cross-state move, while just 0.34 points more likely to switch county within state for the same reason. Secondly, the negative education slopes for the various non-match motivations tend to be larger for within-state moves (with looking for work a notable exception<sup>13</sup>). Unsurprisingly, within-state education effects are also larger for migrating for the sake of an easier commute.

The principal drivers of the negative overall non-match education slope are “other family reasons”<sup>14</sup> (with a 0.12 percentage point gap in cross-state migration propensity between post-graduates and high school dropouts, and 0.21 within-state), cheaper housing (0.04 gap across states and a 0.1 gap within), “other housing reasons”<sup>15</sup> (negligible across states, 0.13 within), better neighbourhood (0.02 across, 0.05 within), and looking for work (0.08 across, 0.07 within). In almost all these cases, the effects are monotonic across education categories. Interestingly, there is only one non-match motivation with a significant positive education slope: the desire to purchase a home.

I will argue below that these negative slopes partly reflect the large surpluses in high skilled job matches: workers are unwilling to forfeit these surpluses to realise gains in amenity value (with amenities broadly defined to include family, housing and local job availability). Of course, it is also plausible that low skilled workers are subject to larger shocks which

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<sup>13</sup>This is intuitive, given that labour market shocks tend to be correlated between nearby localities.

<sup>14</sup>Unfortunately, this “other family” category is not further disaggregated in the CPS data. An important component of this response is likely to be moving closer to relatives. Interestingly, neither of the other family motivations (change of marital status or establishing own household) contribute to the negative slope.

<sup>15</sup>Like the “other family” category, this “other housing reasons” category is not further disaggregated in the CPS.

Table 3.4: Marginal effects from probit regressions on migration incidence

*Dependent variable: takes 1 for a move (of specified motivation) in previous 12 months*

Primary reason	CROSS-STATE				CROSS-COUNTY WITHIN STATE			
	HS grad	Some coll	Coll grad	Post grad	HS grad	Some coll	Coll grad	Post grad
All reasons	-0.102 (0.067)	-0.036 (0.068)	0.383*** (0.069)	0.936*** (0.074)	-0.243*** (0.074)	-0.134* (0.076)	-0.159** (0.079)	-0.249*** (0.089)
MATCH-SPECIFIC								
New job/transfer	0.028 (0.048)	0.213*** (0.048)	0.605*** (0.047)	0.974*** (0.049)	0.049 (0.036)	0.165*** (0.036)	0.280*** (0.036)	0.341*** (0.039)
Commute	-0.002 (0.012)	0.007 (0.012)	0.009 (0.012)	0.034*** (0.013)	0.025 (0.025)	0.067*** (0.025)	0.082*** (0.026)	0.107*** (0.028)
NON-MATCH								
Change in marital status	0.007 (0.015)	0.004 (0.016)	-0.016 (0.017)	-0.001 (0.018)	0.042* (0.025)	0.062** (0.025)	0.018 (0.026)	0.021 (0.029)
Establish own household	-0.002 (0.012)	-0.004 (0.012)	-0.002 (0.013)	-0.005 (0.015)	-0.017 (0.020)	-0.002 (0.020)	-0.031 (0.021)	-0.056** (0.026)
Other family reasons	-0.056** (0.024)	-0.061** (0.025)	-0.068*** (0.026)	-0.124*** (0.031)	-0.054** (0.024)	-0.080*** (0.025)	-0.139*** (0.027)	-0.207*** (0.032)
Want to own home	-0.001 (0.012)	-0.012 (0.013)	0.009 (0.013)	0.01 (0.014)	0.017 (0.024)	0.057** (0.025)	0.079*** (0.025)	0.054* (0.028)
New or better housing	0.01 (0.014)	-0.01 (0.015)	-0.015 (0.016)	-0.021 (0.018)	-0.014 (0.026)	-0.027 (0.026)	-0.022 (0.027)	-0.071** (0.031)
Cheaper housing	-0.014 (0.011)	-0.027** (0.012)	-0.046*** (0.013)	-0.039*** (0.015)	-0.028* (0.016)	-0.053*** (0.016)	-0.065*** (0.017)	-0.110*** (0.022)
Other housing reasons	0.005 (0.016)	-0.02 (0.016)	-0.014 (0.016)	-0.002 (0.018)	-0.068*** (0.019)	-0.097*** (0.020)	-0.118*** (0.021)	-0.133*** (0.025)
Better neighbourhood	-0.015* (0.009)	-0.017* (0.009)	-0.031*** (0.010)	-0.018* (0.010)	-0.022 (0.014)	-0.021 (0.015)	-0.035** (0.015)	-0.051*** (0.018)
Climate, health, retirement	0.015 (0.015)	0.019 (0.015)	0.015 (0.015)	-0.005 (0.017)	-0.001 (0.008)	-0.009 (0.008)	-0.016* (0.009)	-0.003 (0.010)
Look for work	-0.041*** (0.013)	-0.049*** (0.014)	-0.060*** (0.015)	-0.075*** (0.018)	-0.045*** (0.010)	-0.047*** (0.011)	-0.066*** (0.012)	-0.068*** (0.015)
Other reasons	0.015 (0.011)	0.007 (0.011)	0.030*** (0.011)	0.034*** (0.013)	-0.026** (0.013)	-0.029** (0.013)	-0.042*** (0.014)	-0.050*** (0.016)
AMBIGUOUS								
Other job reasons	0.012 (0.016)	0.024 (0.016)	0.043*** (0.016)	0.081*** (0.017)	-0.007 (0.012)	-0.003 (0.012)	-0.009 (0.012)	-0.003 (0.014)

This table reports marginal effects of various education levels from probit regressions on annual migration incidence. Each row reports the effects on moving for the motivation specified, with the first row presenting education effects on the overall migration incidence (all reasons). The first four columns gives results for cross-state moves and the final four for cross-county moves within states. Marginal effects relate to change in variable from 0 to 1; they are scaled by 100 and reported in percentage terms. Estimates are based on panel of CPS cross-sections between 1999 and 2013. The sample is restricted to labour force participants outside the armed forces, aged 25-64. Individuals who move primarily to attend or leave or college or because of natural disasters are also excluded (these account for 2

encourage non-match moves. For example, lower educated workers tend to be more credit constrained, so may pay more attention to housing costs when choosing a location. Also, they are more commonly unemployed, which makes them more likely to move to look for work (though it is clearly significant that so few of them engage in match-specific migration). And family instability and disruption may be more common among low income families (see e.g. McLanahan, 1985). But in any case, the fact that the statistically significant non-match education slopes are almost always negative and monotonic is certainly striking.

An important caveat in interpreting these results is that migration decisions are often made in the context of a household, rather than at the individual level where I have conducted my analysis. Of course, many individuals migrate to meet the needs of another household member. For example, a husband might move state to allow his wife to take up a new job. Now, the CPS question on primary reason for moving is asked of individuals. But, it appears that many individuals simply report the reasons of fellow household members. In the previous example, both the husband and the wife may report moving for the sake of a new job; even though the husband should really report a family-related reason (from an individual perspective). In 68% of multi-person households with at least one member moving for the sake of a new job or job transfer, *all* households members report moving for this same reason.<sup>16</sup> It seems unlikely that, in so many of these households, the change of residence was motivated by *multiple* members finding jobs in the *same* county. In particular, 83% of *children* under 16 in these households also report moving for the sake of a new job.<sup>17</sup> This will not affect the broad conclusions from the analysis above if households are composed of individuals with similar levels of education; but to the extent that this is not the case, this could conflate the results.

To address some of these concerns, I re-estimate the equations of Table 3.4 using a robustness sample. Firstly, to deal with misreported reasons among household dependents, I restrict the sample to the top-earning individual in each household.<sup>18</sup> And secondly, to address the problem of household instability among low income families, I further restrict the sample to top-earners living (1) alone or (2) with a spouse and children only.<sup>19</sup>

The results for the robustness sample are reported in Table 3.5. For comparability, the table structure is identical to Table 3.4. Interestingly, the education slopes of match-specific migration are now significantly steeper: post-graduates are 1.32 percentage points more likely to make a cross-state move, compared to 0.97 in the full sample. Unlike the full sample though, the effect of education on commuting-motivated migration is insignificant. Also, the magnitudes of many of the non-match education effects have held up reasonably well in the robustness sample. In particular, the within-state effect of postgraduate education

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<sup>16</sup>This statistic is based on all household members, including those excluded from the sample under consideration in the analysis above (i.e. the economically inactive, and those under 25 or over 64).

<sup>17</sup>Indeed, the IPUMS documentation comments that “while data [on the primary reason for moving] were collected for all movers age 1 and older, the responses for minors doubtless reflect the rationales given by adults in the household.” See [https://cps.ipums.org/cps-action/variables/WHYMOVE#description\\_section](https://cps.ipums.org/cps-action/variables/WHYMOVE#description_section)

<sup>18</sup>I exclude all households with multiple individuals earning the same top wage and all households with no wage income.

<sup>19</sup>I exclude single parents living with children, in case this is linked with family instability.

(relative to high-school dropouts) is -0.16 for “other family reasons”, compared to -0.21 for the full sample. This suggests this effect is not driven by misreporting of migration reasons or education-differentiated household instability.

### 3.3 Model

#### Overview

I present a continuous-time job matching model with directed search across  $z$  cities, where  $z$  is large. The economy consists of  $n$  workers. Each worker is characterised by a fixed “origin city”, with an equal number of workers assigned to each city. These origins determine each worker’s geographical centre of job search (I explain what this means below); and they are fixed, irrespective of worker’s migration histories. Workers are either employed or unemployed, and the latter receive a flow utility  $b$ . For simplicity, I assume that only the unemployed search for work. Firms are homogeneous and are free to open vacancies anywhere along the circumference. Each firm employs a single worker to produce a single output good, with price normalised to 1.

Productivity is the sum of  $y$  and a random match component  $\sigma\varepsilon$ . I assume  $\varepsilon$  is distributed according to  $F$ , with mean 0 and with hazard rate  $\frac{f(x)}{1-F(x)}$  monotonically increasing in  $x$ <sup>20</sup>. The parameter  $\sigma$  modifies the dispersion of the productivity shock. Matches are consummated if the associated job surplus exceeds zero. In this case, the wage is set according to a Nash bargain.<sup>21</sup> Job separations occur at an exogenous rate  $\delta$ .

I assume that the unemployed know of the existence and location of all vacancies; and recruiting firms have equivalent information on the unemployed. In this sense, workers and firms do not need to look for matches. Instead, there are two frictions that separate a given worker  $i$  and firm  $j$ . Firstly, it is costly to learn the idiosyncratic match productivity. This information is randomly revealed according to a Poisson process defined by a “revelation function”  $q$ . The arguments of this function are (1) the learning effort<sup>22</sup>  $e_{w,ij}$  directed by worker  $i$  to firm  $j$  and (2) the learning effort  $e_{f,ji}$  directed by firm  $j$  to worker  $i$ . For simplicity, I assume a match can only be accepted or rejected once this information is revealed. Learning effort is subject to a cost  $\frac{1}{2}\gamma_\tau^X (e_{\tau,ij})^2$  for  $\tau = \{f, w\}$ . The superscript  $X$  denotes the match type, where  $X = L$  denotes a local match (where the firm’s location coincides with the worker’s origin) and  $X = N$  denotes a non-local match. Non-local effort is more expensive:  $\gamma_\tau^N > \gamma_\tau^L$ . But, because of the convexity of learning costs, firms and workers do exert some effort outside their home city.

The second friction is a matching cost  $m^X$ , paid by the worker on the acceptance of a match, with  $m^L = 0$  and  $m^N > 0$ . In this sense, the matching cost can be understood to

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<sup>20</sup>This property is characteristic of the uniform, normal and exponential distributions.

<sup>21</sup>The precise rent sharing rule is not important, as long as both wages and profits are increasing in job surplus: this ensures that both workers and firms invest resources in learning.

<sup>22</sup>Practically speaking, this would include costs related to advertising, application, interview and any necessary travel.

Table 3.5: Marginal effects from probit regressions on migration incidence: robustness sample

*Dependent variable: takes 1 for a move (of specified motivation) in previous 12 months*

Primary reason	CROSS-STATE				CROSS-COUNTY WITHIN STATE			
	HS grad	Some coll	Coll grad	Post grad	HS grad	Some coll	Coll grad	Post grad
All reasons	-0.106 (0.116)	0.12 (0.117)	0.610*** (0.117)	1.238*** (0.120)	-0.199 (0.123)	0.081 (0.124)	-0.005 (0.126)	-0.111 (0.135)
MATCH-SPECIFIC								
New job/transfer	0.016 (0.093)	0.341*** (0.092)	0.845*** (0.091)	1.317*** (0.093)	0.039 (0.068)	0.229*** (0.068)	0.332*** (0.068)	0.395*** (0.071)
Commute	0.015 (0.024)	0.022 (0.024)	0.025 (0.024)	0.037 (0.025)	-0.024 (0.043)	0.006 (0.042)	0.001 (0.043)	0.036 (0.045)
NON-MATCH								
Change in marital status	-0.019 (0.024)	-0.014 (0.025)	-0.036 (0.026)	-0.023 (0.027)	0.056 (0.041)	0.085** (0.041)	0.026 (0.042)	0.027 (0.045)
Establish own household	-0.016 (0.018)	-0.031* (0.019)	-0.015 (0.019)	-0.026 (0.020)	-0.052 (0.031)	-0.017 (0.031)	-0.054* (0.033)	-0.113*** (0.038)
Other family reasons	-0.046 (0.037)	-0.024 (0.037)	-0.025 (0.038)	-0.092** (0.041)	-0.041 (0.033)	-0.044 (0.034)	-0.090*** (0.035)	-0.157*** (0.040)
Want to own home	0.019 (0.025)	0.012 (0.025)	0.034 (0.026)	0.046* (0.027)	-0.009 (0.041)	0.023 (0.041)	0.058 (0.041)	0.024 (0.044)
New or better housing	-0.004 (0.024)	0.005 (0.024)	-0.01 (0.024)	-0.018 (0.026)	-0.063 (0.043)	-0.05 (0.044)	-0.079* (0.045)	-0.120** (0.047)
Cheaper housing	0.01 (0.020)	-0.002 (0.020)	-0.033 (0.021)	-0.023 (0.023)	-0.022 (0.022)	-0.048** (0.023)	-0.056** (0.024)	-0.104*** (0.028)
Other housing reasons	-0.009 (0.023)	-0.024 (0.024)	-0.028 (0.024)	-0.01 (0.025)	-0.038 (0.031)	-0.042 (0.031)	-0.060* (0.032)	-0.073** (0.035)
Better neighbourhood	-0.019 (0.013)	-0.021 (0.013)	-0.031** (0.013)	-0.018 (0.014)	0.023 (0.025)	0.018 (0.026)	0.003 (0.026)	-0.002 (0.028)
Climate, health, retirement	0.011 (0.021)	0.02 (0.021)	0 (0.022)	0.005 (0.023)	0.006 (0.011)	0.004 (0.012)	-0.002 (0.013)	0.015 (0.013)
Look for work	-0.023 (0.018)	-0.040** (0.019)	-0.042** (0.019)	-0.057*** (0.022)	-0.030** (0.013)	-0.028** (0.014)	-0.042*** (0.015)	-0.031** (0.015)
Other reasons	0.015 (0.017)	0.022 (0.017)	0.043** (0.017)	0.047*** (0.018)	0.003 (0.017)	0.005 (0.017)	0.006 (0.018)	-0.006 (0.020)
AMBIGUOUS								
Other job reasons	0.007 (0.028)	0.006 (0.028)	0.03 (0.027)	0.065** (0.028)	0.028 (0.024)	0.050** (0.023)	0.036 (0.024)	0.027 (0.025)

This table re-estimates the equations of Table 10, but using a robustness sample. Specifically, I further restrict the sample to top-earners in households living (1) alone or (2) with a spouse and child only. I exclude households with joint top earners, and I also exclude parents living alone with children. The sample size in each regression is 513,756. SEs in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

represent a one-off moving expense: a worker who finds a job outside his home city must reside next to the firm (there is no commuting). Once a worker loses his job, he returns to his origin at no cost.

I study the effect of raising (1) the difference between productivity and out-of-work utility  $y - b$  and (2) the match productivity dispersion  $\sigma$ , all else equal, to simulate a higher skilled labour market. I show these markets are tighter and are characterised by greater investment in learning effort. I then derive the implications for responses to local shocks.

## Productivity revelation

The productivity of the match between a given worker  $i$  and firm  $j$  is revealed according to the following Poisson process:

$$q(e_{w,ij}, e_{f,ji}) = \mu e_{w,ij}^\alpha e_{f,ji}^{1-\alpha} \quad (3.1)$$

where  $\mu$  is the revelation efficiency parameter and  $\alpha \in (0, 1)$ .  $e_{w,ij}$  is the learning effort directed by worker  $i$  to firm  $j$ , and  $e_{f,ji}$  is the firm's learning effort directed at worker  $i$ . Given the symmetry of the model, the learning effort of workers can take one of two values, depending on whether the match is local or non-local; and the same is true of firms' effort. Specifically,  $e_{w,ij} = e_w(X)$  and  $e_{f,ji} = e_f(X)$ , where  $X = \{L, N\}$  specifies whether the match is local or non-local. And so, the instantaneous revelation probability between a worker and firm for match type  $X$  can be expressed as:

$$q(X) = \mu e_w(X)^\alpha e_f(X)^{1-\alpha}$$

## Worker and firm values

For a worker of any origin, the value of being unemployed is:

$$rU = b + \sum_{X=\{L,N\}} \max_{e_w} \left\{ \mu e_w(X)^\alpha e_f(X)^{1-\alpha} \hat{U}(X) - \frac{1}{2} \gamma_w^X e_w(X)^2 \right\} \omega^X v \quad (3.2)$$

where, for each match type  $X$ , workers choose the optimal learning effort  $e_w$ . The total stock of vacancies is  $v$ , and this is weighted by  $\omega^X$  which denotes the share of vacancies which are local ( $X = L$ ) or non-local ( $X = N$ ). Given the symmetry of the model,  $\omega^L = \frac{1}{z}$  and  $\omega^N = \frac{z-1}{z}$ , where  $z$  is the number of cities. Workers choose the optimal learning effort  $e_w(X)$ , according to match type  $X = \{L, N\}$ .  $r$  is the discount rate, and  $\hat{U}(X)$  is the expected value to the worker (before the match quality is revealed) of a type  $X$  match. Specifically:

$$\hat{U}(X) = \int_{\varepsilon} \max \{ E(X, \varepsilon) - U - m^X, 0 \} dF \quad (3.3)$$

On discovering the productivity of a type  $X$  match, the worker accepts it if  $E(X, \varepsilon) - U \geq m^X$ , where  $E$  is the employment value and  $m^X$  is the matching cost, which is positive only in a non-local match. The employment value is given by:

$$rE(X, \varepsilon) = w(X, \varepsilon) + \delta(U - E(X, \varepsilon)) \quad (3.4)$$

where  $w(X, \varepsilon)$  is the negotiated wage, given match type  $X$  and a productivity draw of  $\varepsilon$ .

The problem faced by firms is similar. Unlike workers, firms choose the city  $j$  that yields the largest vacancy value<sup>23</sup>:

$$V = \max_j V_j \quad (3.5)$$

In a spatial equilibrium,  $V_j = V$  for all  $j$ , where:

$$rV = \sum_{X=\{L,N\}} \max_{e_f} \left\{ \mu e_w(X)^\alpha e_f(X)^{1-\alpha} \hat{V}(X) - \frac{1}{2} \gamma_f^X e_f(X)^2 \right\} \omega^X u \quad (3.6)$$

The intuition is identical for the value of unemployment: for each match type  $X$ , firms choose the optimal learning effort  $e_f$ .  $u$  is the stock of unemployed, and  $\omega^X$  is the fraction of workers corresponding to match type  $X$ .  $\hat{V}(X)$  is the expected value to the firm of a match of type  $X$ , where:

$$\hat{V}(X) = \int_{\varepsilon} \max \{J(X, \varepsilon) - V, 0\} dF \quad (3.7)$$

The match is consummated if  $J(X, \varepsilon) \geq V$ . The value of a filled job  $J(X, \varepsilon)$  varies with the match type  $X$  because the wage bargain is affected by the matching cost. Specifically,  $J$  is given by:

$$rJ(X, \varepsilon) = y + \sigma\varepsilon - w(X, \varepsilon) + \delta(V - J(X, \varepsilon)) \quad (3.8)$$

Conditional on the productivity draw  $\varepsilon$ , I define the match surplus gross of the matching cost as:

$$\begin{aligned} \Omega(\varepsilon) &= E(X, \varepsilon) - U + J(X, \varepsilon) - V \\ &= \frac{1}{r + \delta} (y + \sigma\varepsilon - r(U + V)) \end{aligned} \quad (3.9)$$

A type  $X$  match between a worker and firm is accepted if  $\Omega(\varepsilon) \geq m^X$ , or equivalently if  $\varepsilon \geq \tilde{\varepsilon}(X)$ , where:

$$\tilde{\varepsilon}(X) = \frac{rU + rV - y + (r + \delta)m^X}{\sigma}$$

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<sup>23</sup>Notice I do not grant workers a similar choice over origin cities. But despite this, the value of unemployment is still invariant across origins in equilibrium (because of the symmetry of the model). Consequently, this equilibrium would still exist if I allowed workers to choose their origin. However, that equilibrium would be unstable because workers and firms would be better off if they all clustered at a single location, minimising search and matching costs. In this framework, I could ensure stability by incorporating diminishing returns to locations (whether through the local production function, congestion externalities or imperfectly elastic housing supply). But since this is not my focus, I keep the model simple and assume fixed origins.

In this case, the surplus net of the matching cost is shared according to a Nash bargain:

$$E(X, \varepsilon) - U - m^X = \phi [\Omega(\varepsilon) - m^X] \quad (3.10)$$

where  $\phi$  denotes the bargaining power of workers. The equilibrium wage can be derived by substituting equation (3.4) for  $E(\varepsilon, \lambda)$  in the Nash bargain:

$$w(X, \varepsilon) = \phi(y + \sigma\varepsilon - rV) + (1 - \phi)(m^X + rU) \quad (3.11)$$

## Learning effort choices

Given a match type  $X$ , the first order conditions for workers' and firms' learning effort are:

$$e_w(X) = \frac{\mu\alpha\phi}{\gamma_w^X} \left( \frac{e_f(X)}{e_w(X)} \right)^{1-\alpha} \int_{\varepsilon} \max\{\Omega(\varepsilon) - m^X, 0\} dF \quad (3.12)$$

and

$$e_f(X) = \frac{\mu(1-\alpha)(1-\phi)}{\gamma_f^X} \left( \frac{e_f(X)}{e_w(X)} \right)^{-\alpha} \int_{\varepsilon} \max\{\Omega(\varepsilon) - m^X, 0\} dF \quad (3.13)$$

respectively. Clearly, learning effort is larger for local matches. This is because (1) the learning cost is smaller ( $\gamma^L < \gamma^N$ ) and (2) no matching cost  $m$  is paid.

Also, these two equations yield a simple expression for relative learning effort of workers and firms:

$$\frac{e_w(X)}{e_f(X)} = \sqrt{\frac{\alpha}{1-\alpha} \frac{\phi}{1-\phi} \frac{\gamma_f^X}{\gamma_w^X}} \quad (3.14)$$

The relative effort will vary with match type  $X$  if either workers or firms have a comparative advantage in non-local learning. Substituting this back into the first order conditions yields:

$$e_w(X) = \frac{\mu\alpha\phi}{\gamma_w^X} \left( \frac{1-\alpha}{\alpha} \frac{1-\phi}{\phi} \frac{\gamma_w^X}{\gamma_f^X} \right)^{\frac{1-\alpha}{2}} \int_{\varepsilon} \max\{\Omega(\varepsilon) - m^X, 0\} dF \quad (3.15)$$

and

$$e_f(X) = \frac{\mu(1-\alpha)(1-\phi)}{\gamma_f^X} \left( \frac{\alpha}{1-\alpha} \frac{\phi}{1-\phi} \frac{\gamma_f^X}{\gamma_w^X} \right)^{\frac{\alpha}{2}} \int_{\varepsilon} \max\{\Omega(\varepsilon) - m^X, 0\} dF \quad (3.16)$$

Notice that applying the first order conditions to equations (3.2) and (3.3) gives:

$$rU = b + \frac{2-\alpha}{2\alpha} \sum_{X=\{L,N\}} \gamma_w^X e_w(X)^2 \omega^X v \quad (3.17)$$

in equilibrium. Similarly, applying them to equations (3.6) and (3.7):



$$rV = \frac{1 + \alpha}{2(1 - \alpha)} \sum_{X=\{L,N\}} \gamma_f^X e_f(X)^2 \omega^X u \quad (3.18)$$

And combining equations (3.17) and (3.18):

$$\frac{rU - b}{rV} = \frac{2 - \alpha}{1 + \alpha} \frac{\phi}{1 - \phi} \frac{v}{u} \quad (3.19)$$

where the ratio of the unemployment and vacancy values is increasing in the tightness of the labour market,  $\frac{v}{u}$ .

## Equilibrium

The job finding rate  $\rho$  for unemployed workers is given by:

$$\rho = \mu \sum_{X=\{L,N\}} e_w(X)^\alpha e_f(X)^{1-\alpha} [1 - F(\tilde{\varepsilon}(X))] \omega^X v \quad (3.20)$$

so the equilibrium unemployment rate is:

$$\frac{u}{n} = \frac{\delta}{\delta + \mu \sum_{X=\{L,N\}} e_w(X)^\alpha e_f(X)^{1-\alpha} [1 - F(\tilde{\varepsilon}(X))] \omega^X v} \quad (3.21)$$

I have so far described six key equations: (3.17), (3.18), (3.9), (3.15), (3.16) and (3.21). But, these contain seven unknowns:  $U$ ,  $V$ ,  $\Omega(\varepsilon, \lambda)$ ,  $e_w(\lambda)$ ,  $e_f(\lambda)$ ,  $u$  and  $v$ . To complete the system, I impose a free entry condition. Suppose the cost of opening a vacancy is fixed at  $\bar{V}$ , so firms have an incentive to enter the economy as long as  $V \geq \bar{V}$ . In equilibrium, the following condition must be satisfied:

$$V = \bar{V} \quad (3.22)$$

The equilibrium wage  $w(\varepsilon, \lambda)$  can then be solved as a function of the unknowns above using equation (3.11).

The model has a constant returns to scale property: holding the number of cities  $z$  (and hence the  $\omega^X$  weights) fixed, the equilibrium outcomes are invariant to the worker population  $n$ . But, based on equation (3.21), the unemployment rate is increasing in the number of cities.<sup>24</sup> This arises from an agglomeration effect familiar from the urban economics literature, where higher quality matches materialise more efficiently in more densely populated areas or larger cities; see Helsley and Strange (1990).

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<sup>24</sup>As the number of cities  $z$  grows,  $\omega^N$  becomes larger relative to  $\omega^L$ . But of course, non-local search of course entails greater frictions (in terms of learning and matching costs); so fewer matches are formed.

## 3.4 Equilibrium results

### Hazard rate dominance

Let  $G$  be the distribution of  $\Omega(\varepsilon)$ , the job surplus gross of matching cost. Specifically:

$$\begin{aligned} G(x) &= \Pr(\Omega(\varepsilon) \leq x) \\ &= \Pr\left(\varepsilon \leq \frac{rU + rV - y + (r + \delta)x}{\sigma}\right) \\ &= F\left(\frac{rU + rV - y + (r + \delta)x}{\sigma}\right) \end{aligned} \quad (3.23)$$

Then, it can be shown that, for any given surplus  $x$ , the hazard rate  $\frac{g(x)}{1-G(x)}$  is:

1. Decreasing in  $y-b$ , the difference between average productivity and out-of-work benefit
2. Decreasing in  $\sigma$ , the dispersion of match productivity (I do not yet have this proof)

What is the significance of this result? Suppose a high skilled economy is characterised by relatively large  $y-b$  and/or large  $\sigma$ . Then, the job surplus distribution in a high skilled economy will dominate that of a low skilled economy, according to the hazard rate criterion. The existence of large job surpluses has important implications for job creation, search behaviour and mobility, as I demonstrate below.

The first statement, on the impact of changes in  $y-b$ , is simple to prove. Notice that:

$$\frac{g(x)}{1-G(x)} = \frac{r + \delta}{\sigma} \cdot \frac{f\left(\frac{(rU-b)+rV-(y-b)+(r+\delta)x}{\sigma}\right)}{1 - F\left(\frac{(rU-b)+rV-(y-b)+(r+\delta)x}{\sigma}\right)} \quad (3.24)$$

The term  $\frac{(rU-b)+rV-(y-b)+(r+\delta)x}{\sigma}$  represents the match productivity  $\varepsilon$  which guarantees a gross surplus  $x$ . As  $y-b$  grows, this term will contract (for given  $x$ ). This is because part of the gains from the larger productivity are not captured by workers and firms (in the sum of  $rU$  and  $rV$ ), since workers are not always employed and vacancies are not always filled. And, since I have assumed the hazard rate of  $F$  is monotonically increasing, equation (3.24) shows that  $\frac{g(x)}{1-G(x)}$  must decrease for all  $x$ .

Next, consider the second statement on the impact of  $\sigma$ . Intuitively, job surpluses must grow: given the possibility of match rejection, workers and firms only take the upside of the larger dispersion. Unfortunately, I have not yet been able to prove hazard rate dominance formally. Still, I do believe such a proof is feasible - and, for the remainder of this chapter, I assume that hazard rate dominance holds as  $\sigma$  grows.

### Effect of $y-b$ and $\sigma$ on learning effect and market tightness

The hazard rate dominance result implies that, as  $y-b$  and  $\sigma$  grow, the surplus value of a job match  $\int_{\varepsilon} \max\{\Omega(\varepsilon) - m^X, 0\}$  will be larger for both local ( $X = L$ ) and non-local

( $X = N$ ) matches. It immediately follows from equations (3.15) and (3.16) that learning effort directed at each individual firm and worker,  $e_w(X)$  and  $e_f(X)$ , must also be larger for each match type  $X$ .

I next derive the implications for the unemployment stock and unemployment value. Since (1) the vacancy value  $V$  is fixed at  $\bar{V}$  and (2)  $e_f(X)$  is larger for each  $X$ , equation (3.18) implies that the unemployment stock  $u$  must be smaller in equilibrium (and the job finding probability must be larger). Also, while  $V$  is fixed, the larger match surplus is manifested in a larger unemployment value  $U$ .<sup>25</sup>

Equation (3.19) then demonstrates that a larger  $U$  and fixed  $V$  implies that market tightness  $\frac{v}{u}$  is larger. Interestingly, while firms direct more learning effort at each individual worker ( $e_f(X)$  is larger for each match type  $X$ ), equation (3.18) shows that the *total* expenditure by individual firms on learning,  $\sum_{X=\{L,N\}} \gamma_f^X e_f(X)^2 \omega^X u$ , is no larger in high skilled markets (since  $V$  is fixed). Instead, firms exhaust the larger job surplus through *entry*: this explains the tighter labour market.<sup>26</sup> Workers, in contrast, respond by increasing their learning expenditure  $\int_X \gamma_w^X e_w(X)^2 \omega^X v$  (implied by the larger  $U$  in equation (3.17)). Intuitively, this is because the number of workers is fixed by assumption.

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<sup>25</sup>The formal proof is as follows. The probability of finding a job,  $\rho$ , is:

$$\begin{aligned} \rho &= \mu \sum_{X=\{L,N\}} e_w(X)^\alpha e_f(X)^{1-\alpha} [1 - G(m^X)] \omega^X v \\ &= \mu^2 \left( \frac{\alpha \phi}{\gamma_w^X} \right)^\alpha \left( \frac{(1-\alpha)(1-\phi)}{\gamma_f^X} \right)^{1-\alpha} \sum_{X=\{L,N\}} \int_\varepsilon \max\{\Omega(\varepsilon) - m^X, 0\} [1 - G(m^X)] \omega^X v \end{aligned}$$

where the second line follows after substituting the first order conditions. Next, substituting the first order condition for  $e_w$  into equation (3.17) gives:

$$\begin{aligned} rU &= b + \lambda \sum_{X=\{L,N\}} \left[ \int_\varepsilon \max\{\Omega(\varepsilon) - m^X, 0\} dF \right]^2 \omega^X v \\ &= b + \lambda \sum_{X=\{L,N\}} \left[ \int_\varepsilon \max\{\Omega(\varepsilon) - m^X, 0\} dF \right] \cdot [1 - G(m^X)] \cdot \mathbb{E}[\Omega(\varepsilon) - m^X | \Omega(\varepsilon) \geq m^X] \omega^X v \end{aligned}$$

where

$$\lambda = \frac{\alpha(2-\alpha)\mu^2\phi^2}{\gamma_w^X} \left( \frac{1-\alpha}{\alpha} \frac{1-\phi}{\phi} \frac{\gamma_w^X}{\gamma_f^X} \right)^{1-\alpha}$$

and  $\mathbb{E}[\Omega(\varepsilon) - m^X | \Omega(\varepsilon) \geq m^X]$  is the expectation of the job surplus *conditional* on the match being accepted. Now, the expected conditional surplus must increase with  $y - b$  and  $\sigma$  according to the hazard rate dominance result. And, since I have shown that the unemployment stock  $u$  falls, the job finding rate  $\rho$  and the expression  $\sum_{X=\{L,N\}} \int_\varepsilon \max\{\Omega(\varepsilon) - m^X, 0\} [1 - G(m^X)] \omega^X v$  must also increase. It follows that the unemployment value  $U$  must grow.

<sup>26</sup>Intuitively, in a world where entry is more restricted, the effect on market tightness will be smaller, and individual firms will increase their total learning expenditure.

## Effect of $y - b$ and $\sigma$ on match-specific migration

Let  $\rho^X$  be the probability of finding a job of match type  $X$ . Then:

$$\begin{aligned}\rho^X &= \mu e_w (X)^\alpha e_f (X)^{1-\alpha} [1 - G(m^X)] \omega^X v \\ &= \mu^2 \left( \frac{\alpha \phi}{\gamma_w^X} \right)^\alpha \left( \frac{(1-\alpha)(1-\phi)}{\gamma_f^X} \right)^{1-\alpha} \int_\varepsilon \max \{ \Omega(\varepsilon) - m^X, 0 \} [1 - G(m^X)] \omega^X v \\ &= \mu^2 \left( \frac{\alpha \phi}{\gamma_w^X} \right)^\alpha \left( \frac{(1-\alpha)(1-\phi)}{\gamma_f^X} \right)^{1-\alpha} [1 - G(m^X)]^2 \cdot \mathbb{E} [\Omega(\varepsilon) - m^X | \Omega(\varepsilon) \geq m^X] \omega^X v\end{aligned}\tag{3.25}$$

where  $\mathbb{E} [\Omega(\varepsilon) - m^X | \Omega(\varepsilon) \geq m^X]$  is the expectation of the job surplus *conditional* on the match being accepted. The model is ambiguous on whether  $\rho^N$  or  $\rho^L$  is larger. On the one hand, non-local matches are subject to a matching cost  $m$  and larger learning costs  $\gamma_w$  and  $\gamma_f$ . On the other hand, most vacancies are non-local ( $\omega^N > \omega^L$ ). Clearly though, the former effect dominates empirically, so  $\rho^L > \rho^N$ . The odds ratio of finding a non-local, relative to a local, job is:

$$\frac{\rho^N}{\rho^L} = \left( \frac{\gamma_w^L}{\gamma_w^N} \right)^\alpha \left( \frac{\gamma_f^L}{\gamma_f^N} \right)^{1-\alpha} \frac{\omega^N}{\omega^L} \left[ \frac{1 - G(m)}{1 - G(0)} \right]^2 \frac{\mathbb{E} [\Omega(\varepsilon) - m | \Omega(\varepsilon) \geq m]}{\mathbb{E} [\Omega(\varepsilon) | \Omega(\varepsilon) \geq 0]}\tag{3.26}$$

Based on the hazard rate dominance result, it can be shown that the odds ratio  $\frac{\rho^N}{\rho^L}$  is increasing in  $y - b$  and  $\sigma$ . This statement can be proven by showing that both  $\frac{1-G(m)}{1-G(0)}$  and  $\frac{\mathbb{E}[\Omega(\varepsilon)-m|\Omega(\varepsilon)\geq m]}{\mathbb{E}[\Omega(\varepsilon)|\Omega(\varepsilon)\geq 0]}$  are increasing (i.e. becoming closer to 1) in  $y - b$  and  $\sigma$ .<sup>27</sup> Notice that these conditions also imply that the ratio of non-local to local learning effort,  $\frac{e_w^N}{e_w^L}$  and  $\frac{e_f^N}{e_f^L}$ , are also increasing (again, moving closer to 1) in  $y - b$  and  $\sigma$ .

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<sup>27</sup>Consider first the term  $\frac{1-G(m)}{1-G(0)} < 1$ . The elasticity of the acceptance probability (given a matching cost  $m$ ),  $1 - G(m)$ , with respect to the matching cost  $m$  is:

$$\frac{\delta [1 - G(m)]}{\delta m} \cdot \frac{m}{1 - G(m)} = - \frac{g(m)}{1 - G(m)} m$$

Clearly, this elasticity is negative. Since the hazard rates  $\frac{g(m)}{1-G(m)}$  are decreasing in  $y - b$  and  $\sigma$  for all  $m$ , the elasticity of  $1 - G(m)$  must be increasing (i.e. becoming less negative). Consequently, the term  $\frac{1-G(m)}{1-G(0)}$  must also be increasing (i.e. becoming closer to 1). Next, consider the term  $\frac{\mathbb{E}[\Omega(\varepsilon)-m|\Omega(\varepsilon)\geq m]}{\mathbb{E}[\Omega(\varepsilon)|\Omega(\varepsilon)\geq 0]} < 1$ . Firstly, notice that the conditional expected surplus (given a matching cost  $m$ ) can be expressed as:

$$\mathbb{E} [\Omega(\varepsilon) - m | \Omega(\varepsilon) \geq m] = \frac{\int_m^\infty x g(x) dx}{1 - G(m)} - m$$

The elasticity of the conditional surplus with respect to  $m$  is then:

$$\frac{\partial \mathbb{E} [\Omega(\varepsilon) - m | \Omega(\varepsilon) \geq m]}{\partial m} \cdot \frac{m}{\mathbb{E} [\Omega(\varepsilon) - m | \Omega(\varepsilon) \geq m]} = - \left[ 1 + \frac{g(m)}{1 - G(m)} m \right] \frac{m}{\mathbb{E} [\Omega(\varepsilon) - m | \Omega(\varepsilon) \geq m]}$$

It follows then that the “match-specific” migration rate  $\rho^N$  (i.e. the migration rate due to cross-city job matching) must be increasing in  $y - b$  and  $\sigma$ . This is for two reasons. Firstly, as I showed in the previous subsection, the overall job finding rate is increasing in  $y - b$  and  $\sigma$ . And secondly, the odds ratio result implies that a larger proportion of these job finds are non-local.

The intuition is as follows. Larger  $y - b$  and  $\sigma$  generate larger job surpluses; so the non-local matching cost  $m$  will act as a weaker deterrent to long-distance matching. Notice that this result is entirely dependent on the matching cost friction: if this friction did not exist, the non-local odds ratio  $\frac{\rho^N}{\rho^L}$  would be unaffected by  $y - b$  and  $\sigma$ , even if learning costs were increasing in distance. In such a world, workers and firms would be just as happy to post with a local match compared to a non-local one (all else equal). Consequently, larger surpluses would have no effect on the *relative* value of non-local and local matches - and so, they would have no effect on the relative learning effort or job finding rates.

## 3.5 Response to local shocks

### Local productivity shock

Consider an adverse demand shock in some city  $c$ . What does the model say about how migratory responses vary by skill group? The largest hurdle in this exercise is specifying the form of such a shock. It can presumably be approximated as some downward shift in the local match productivity distribution; so the local average productivity, denoted  $y_c$ , will fall. But, the impact of a “typical” shock on the local match dispersion  $\sigma_c$  is unclear. Furthermore, the shift in this productivity distribution is likely to vary across skill groups: for example, Hoynes (2002) makes the point that the larger business cycle volatility observed in low skilled labour market outcomes can partly be explained by the industries in which they are typically employed.

Given these uncertainties, it is perhaps useful to study the migratory response to *given* contraction of local *employment*. To ensure this employment shock is exogenous, I consider the extreme case of the destruction of *all* local jobs, caused by a drop in  $y_c$  to  $-\infty$ . Clearly, since no local matches are viable, the migratory inflow to city  $c$  drops to zero. But, following such a shock, how does the migratory outflow response of origin  $c$  workers vary with  $y - b$  and  $\sigma$ ? - and equivalently, what are the implications for unemployment of origin  $c$  workers?

The unemployment rate of origin  $c$  workers is:

$$\frac{u_c}{n_c} = \frac{\delta}{\delta + \rho_c^L + \rho_c^N} \quad (3.27)$$

where  $n_c$  is the stock of origin  $c$  workers, and  $u_c$  is the number who are unemployed. Following the shock, the local finding rate  $\rho_c^L$  goes to zero. All else equal, the impact on unemployment

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Since (1) the hazard rates are decreasing in  $y - b$  and  $\sigma$  and (2) the conditional expected surplus is increasing, the elasticity of the conditional expected surplus must be growing (i.e. becoming less negative). And so, the term  $\frac{\mathbb{E}[\Omega(\varepsilon) - m | \Omega(\varepsilon) \geq m]}{\mathbb{E}[\Omega(\varepsilon) | \Omega(\varepsilon) \geq 0]}$  must also be increasing (i.e. becoming closer to 1).

is larger in low skilled markets: in this case, a larger proportion of matches would usually be local (the odds ratio  $\frac{\rho_c^N}{\rho_c^L}$  is smaller), so the destruction of local jobs causes more unemployment. The effect is somewhat (though not entirely) moderated by local feedback effects. Specifically, workers with origins in low  $y - b$  and  $\sigma$  economies will suffer a larger decline in their unemployment value,  $U_c$ . This will trigger a larger increase in the job surplus from non-local jobs - and a larger upward response of non-local learning effort  $e_w^N$  and job-finding  $\rho_c^N$ .

I have so far assumed workers' origins, which represent bases for job search, are fixed. But, suppose workers can switch origins, even if this adjustment is costly and sluggish. In the example above, given the larger local disparity in local unemployment values in low skilled markets, we might expect a larger response in terms of origin switches. And, this is consistent with the finding that low skilled workers are more likely to move city to look for work: the job surpluses are too small to sustain substantial match-specific migration, so this sort of "non-match" move may be the only exit route.

## Local amenity shock

Suppose now that city  $c$  receives an adverse amenity shock  $-a_c$ , which takes the form of a utility flow for all unemployed workers of origin  $c$  and all workers employed in the city. As with the shock to productivity, this reduces the set of viable matches in city  $c$ . But, notice that the effect on migratory inflows and outflows will be smaller in high skilled economies with larger  $y - b$  and  $\sigma$ . Again, given the hazard rate dominance result, job surpluses are larger - so fewer matches are made unviable by the shock. Intuitively, given the large job surpluses, workers care less about local amenities when choosing their job. This can help explain why amenity-driven "non-match" moves are more common for the low skilled.

## 3.6 Evidence on the productivity process

In this chapter, I argue that the high employment and migration rates experienced by skilled workers depend fundamentally on two key features of the productivity process: (1) that labour productivity is larger (relative to the out-of-work utility flow) in higher skilled markets; and (2) that dispersion in match productivity is also larger. Both of these contribute to larger job surpluses. In this section, I draw on the Survey of Income and Program Participation (SIPP) for evidence of these features.

For my purposes, the SIPP has three attractive features. Firstly, it is nationally representative sample. Secondly, the samples are very large: the latest panel (beginning 2008) covers 126,000 individuals. Another popular panel survey, the Panel Study of Income Dynamics (PSID), covered 9,000 families in its latest wave; and the National Longitudinal Survey of Youth (NLSY) followed 13,000 individuals. The SIPP panels are limited by short time-frames; but in this chapter, I am only interested in short-term transitions. And thirdly, unlike the PSID or the NLSY, the SIPP records data at high frequencies.<sup>28</sup> In particular,

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<sup>28</sup>PSID respondents were interviewed annually from 1968 until 1999, and every two years since then. NLSY

waves of the SIPP are just four months apart; and in the interview at the end of each wave, survey participants are asked to report information (including wages) for each month since the previous interview. These relatively frequent interviews should reduce the measurement error attributable to memory recall. And more importantly, this frequency allows me to better distinguish wage shocks that are due specifically to job transitions: this is essential for identifying the dispersion of match productivity.

To improve my sample size, I combine three (pre-Great Recession) SIPP samples: 1996, 2001 and 2004: these cover the period from March 1996 to December 2007. I restrict my analysis to the month at the end of each wave (i.e. outcomes contemporaneous to the interview), rather than studying variation between months in the same wave. This should reduce the incidence of measurement error due to poor recall. In particular, it is known that the SIPP suffers from severe seam bias (see e.g. Marquis and Moore, 2010): monthly changes in individuals' outcomes (whether employment status or wages) tend to be larger between months at the seam of two waves than between months within the same wave.

I study active labour force participants aged 25 to 64. When estimating wages, I only look at full-time employees (at least 40 hours per week) still working at the end of the wave; and I exclude those with other jobs or businesses in the final month of the wave. I use hourly wage data for workers paid by the hour, and I estimate hourly wages for salaried workers using monthly earnings and monthly hours. I drop wage observations under \$5 in 2000 prices and top-coded observations .

The first claim that skilled workers benefit from larger labour productivity relative to out-of-work utility is trivial. It is clear that higher skilled labour is significantly more productive; and indeed, the wage gap has been growing since the late 1970s (see e.g. Autor et al., 2008). In my SIPP sample, high-school dropouts earn \$10.37 per hour in 2000 dollars on average, high-school graduates earn \$12.73, workers with some college \$14.91, college graduate \$20.28, and post-graduates \$24.90. Of course, out-of-work utility is unobserved. But, it seems intuitive that it varies less across education groups than earnings: in particular, replacement ratios are smaller at higher incomes<sup>29</sup>.

The second claim is on the dispersion of job match productivity facing a given individual. An important question is whether this dispersion is measured over productivity *levels* or *logarithms*; and similarly, whether worker and firm utility is linear or logarithmic. In the model below, I assume linear utility, with search effort costs and migration costs invariant across skill groups. In this case, larger job surpluses in *absolute* terms will be sufficient to ensure that higher skilled workers invest more job search effort and migrate more. As well as being analytically simpler, this approach has some foundation in the literature: Grogger and Hanson (2011) show that a Roy model with linear utility and skill-invariant migration costs can better explain the observed selection of high and low skilled migrants across countries than an alternative specification with log utility and migration costs which are proportional to income. However, it is not clear whether this result for *international* migration is generalisable

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respondents are interviewed annually.

<sup>29</sup>According to 2012 data from the OECD, single earners in the US earning 67% of the average wage were subject to an initial net replacement rate of 60%; this compares to 32% for those earnings 150% of the average wage. See [http://www.oecd.org/els/soc/NRR\\_Initial\\_EN.xlsx](http://www.oecd.org/els/soc/NRR_Initial_EN.xlsx)

to *internal* migration in the US.

Now, it is entirely intuitive that higher skilled workers are subject to larger match dispersion in *absolute* terms, simply because their wages are significantly higher. Rather than test this hypothesis, I study the more demanding claim that skilled workers face larger dispersion in *log* productivity; and I show that they indeed do. Consider the following wage bargaining rule:

$$\log w_{ig} = \phi_g \log y_{ig} + (1 - \phi_g) \log w_{ig}^R \quad (3.28)$$

where the wage  $w_{ig}$  of individual  $i$  in skill group  $g$  is expressed as a function of labour productivity  $y_{ig}$  and the worker's reservation wage  $w_{ig}^R$ . Consider variation in  $w_{ig}$  driven by match productivity offers received by a searching worker. Taking differences with an individual's expected wage  $w_{ig}^*$  and productivity  $y_{ig}^*$  gives:

$$\log w_{ig} - \log w_{ig}^* = \phi_g (\log y_{ig} - \log y_{ig}^*) \quad (3.29)$$

And taking variances within skill group  $g$ :

$$\text{Var}_g (\log w_{ig} - \log w_{ig}^*) = \phi_g^2 \text{Var}_g (\log y_{ig} - \log y_{ig}^*) \quad (3.30)$$

I am interested in how the parameter  $\text{Var}_g (\log y_{ig} - \log y_{ig}^*)$  varies across skill groups  $g$ . If I assume that the bargaining parameter  $\phi$  varies *relatively* little across skill groups, then the effect of skill on  $\text{Var}_g (\log w_{ig} - \log w_{ig}^*)$  can serve as a proxy for the effect on  $\text{Var}_g (\log y_{ig} - \log y_{ig}^*)$ . But of course, I do not observe the wage offer distribution facing job-hunting individuals. If I wish to study wage variation within individuals, I am restricted to looking at variation in *accepted* offers over time. That is, I proxy  $\text{Var}_g (\log w_{ig} - \log w_{ig}^*)$  with  $\text{Var}_g (\Delta^{job} \log w_{igj})$ , where  $\Delta^{job} \log w_{igj}$  denote the growth in an individual's wage between a job  $j$  and his previous job. This proxy will be suitable as long as skill groups facing relatively large dispersion in job-to-job wage transitions are also subject to large dispersion in wage offers.

Figure 3.6 reports variances in wave-to-wave log wage changes for full-time workers within different age/education groups in my SIPP sample. That is, I study  $\text{Var}_g (\log w_{igt} - \log w_{igt-1})$ , where  $t$  denotes waves of 4-month frequency. I plot these by age group separately for wage shocks involving a job change and wage shocks within jobs. There is a clear positive education gradient for both, but the effect for job changes is much larger. Within the same job, the variances grows from about 0.03 for high school dropouts to 0.08 for postgraduates; and for job changes, the variance grows from about 0.1 to between 0.2 and 0.3 for postgraduates (depending on age group). Of course, wage growth corresponding to job changes matters more for job surplus and matching behaviour. But, where workers are forward-looking, variances over wage growth *within* jobs are also relevant to match quality dispersion.

One possible concern is wage imputation. The Census Bureau imputes wage and earnings data when these are missing using statistical matching on the cross-sectional dimension. Observations based at least partially on imputed data account for 21% of my wage growth





Figure 3.6: Variance of 4-month changes in log full-time hourly wages (SIPP 1996-2007)



Figure 3.7: Variance of 4-month changes in log full-time hourly wages (SIPP 1996-2007): non-imputed data

sample. Since these imputations are not conditional on wage mobility within education groups (the object of interest), they may confound my results. However, the share of these observations varies little across education groups: 22% for high school dropouts, compared to 19% for postgraduates. In Figure 3.7, I reprocess the results excluding all imputed observations. Unsurprisingly, the variances are significantly smaller for the non-imputed sample: they are about half the size within each demographic group. However, apart from this general scaling effect, the patterns look very similar to before.

Of course, there is already a mature literature on earnings processes, and several studies have estimated these separately by education group. The focus is usually on the change in earnings instability over time, and an important concern in this respect has been to distinguish between permanent and transitory earnings components. In the most simple specification (implemented by the seminal study by Gottschalk et al., 1994), the permanent component is estimated as an individual's average earnings over an extended period of time

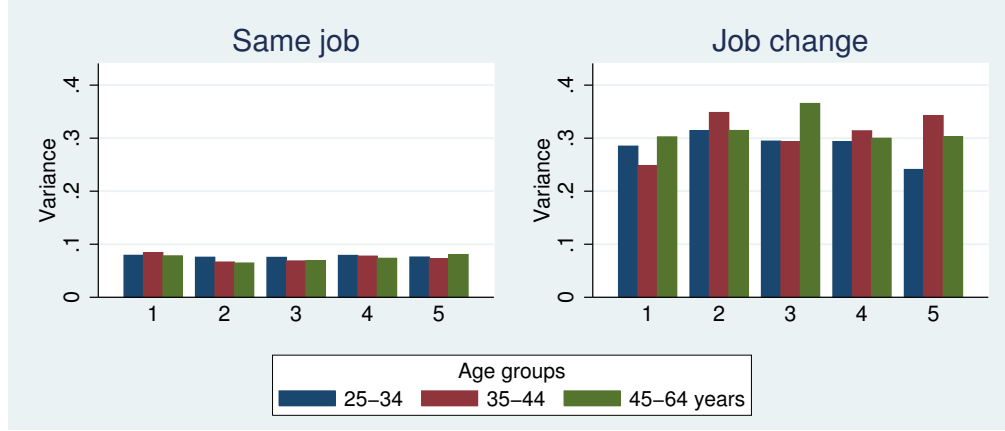


Figure 3.8: Variance of 4-month changes in log monthly earnings (SIPP 1996-2007)

(equivalent to  $w_{igt}^*$ ); and transitory earnings are the deviation from this mean.<sup>30</sup> Using data from the PSID, Gottschalk et al. (1994) show that lower skilled workers faced a significantly larger variance in the transitory component of annual earnings in the 1970s and 1980s; though this gap was reversed from the 1990s (Gottschalk and Moffitt, 2009), at least in binary comparisons between workers with less than 12 years of education and those with more.

It turns out that the lack of education gradients can be explained by the choice of earnings variables: this literature tends to study annual earnings, but I am interested in hourly wages. I confirm this in Figure 3.8, where I plot variances in log changes in monthly earnings (measured in the final month of each wave). The sample is identical to before, but this time I do not exclude part-time workers. I use reported monthly earnings for full-time workers, which I estimate as the product of hourly pay and monthly hours for workers paid by the hour. The variances are now flat across skill groups. These findings are consistent Fitzgerald (1999), who shows (using SIPP panels covering the 1980s and early 1990s) that better educated workers face much larger transitory variance in hourly wages, but not monthly earnings. Here, I have built on Fitzgerald's work by disaggregating wage innovations by job change status.

<sup>30</sup>The object I study is somewhat different from Gottschalk et al. (1994). They use the following model:

$$\log w_{igt} = \log w_{ig}^* + \varepsilon_{igt}$$

where  $\log w_{ig}^*$  is the permanent earnings of individual  $i$  in demographic group  $g$ , and  $\varepsilon_{igt}$  is the transitory component. They estimate the variance of  $\varepsilon_{igt}$  separately for each individual and then take the mean of these variances across individuals in group  $g$ :  $\mathbb{E}_g [\text{Var}_{igt}(\varepsilon_{igt})]$ . I remove the permanent component by computing differences in earnings  $\Delta \log w_{igt} = \Delta \varepsilon_{igt}$ , and I study the variance in  $\Delta w_{igt}$  across all individuals and time periods in group  $g$ :  $\text{Var}_g(\Delta \varepsilon_{igt})$ . The latter object is more useful for this chapter because I am interested in the variance of shocks arising from job *changes*, so it is more natural to look at the variance of  $\Delta \varepsilon_{igt}$  than  $\varepsilon_{igt}$ .

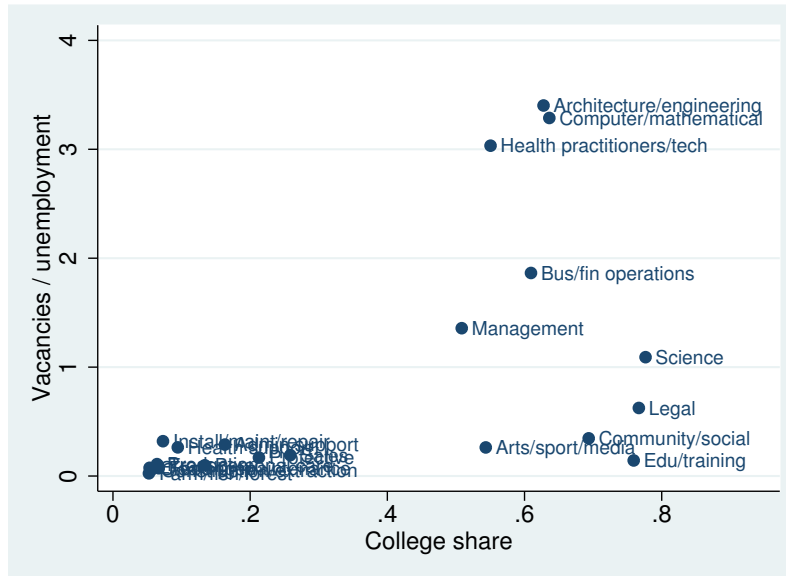


Figure 3.9: Ratio of online job ads to unemployment (Conference Board, Apr 2007; ACS 2007)

### 3.7 Market tightness and search behaviour

Assuming free entry, the model predicts that skilled labour markets should be tighter (i.e. larger  $\frac{v}{u}$ ). I test this using the Conference Board's Help Wanted Online (HWOL) data series. Every month, the Conference Board reports the number of new online job ads and ads reposted from the previous month on 16,000 online job boards.<sup>31</sup> The HWOL data is also disaggregated by occupational SOC classification. I use a (pre-recession) data release from April 2008<sup>32</sup> and take the ratio of these vacancy counts to occupational unemployment estimates from the IPUMS American Community Survey (ACS) of 2007<sup>33</sup>. In the ACS, unemployed workers were asked to report their most recent occupation. In Figure 3.9, I plot these  $\frac{v}{u}$  ratios on occupational college employment share (also from the ACS). For low skilled occupations (with college share below 40%), market tightness ranges from 0.02 to 0.32; and this range is 0.14 to 3.40 for occupations with more than 40% college employment.<sup>34</sup> An important concern with this data is that online job ads clearly do not represent the universe of occupations; and it is plausible that higher skilled jobs are more likely to be advertised online. However, it does not seem plausible that this can explain the very large effect of skill evident from Figure 3.9.

Learning effort is an abstract concept, and as a result, difficult to quantify. But, some

<sup>31</sup>See [https://www.conference-board.org/pdf\\_free/press/TechnicalPDF\\_5202\\_1401797782.pdf](https://www.conference-board.org/pdf_free/press/TechnicalPDF_5202_1401797782.pdf) for further detail.

<sup>32</sup>[http://www.conference-board.org/pdf\\_free/HWOnLine043007\\_PR.pdf](http://www.conference-board.org/pdf_free/HWOnLine043007_PR.pdf)

<sup>33</sup>The IPUMS ACS data was compiled by Ruggles et al. (2010a)

<sup>34</sup>The tightness range for skilled occupations is large. It is plausible that the loose markets that characterise the arts/sport/media or education/training categories are attributable to restricted firm entry in those industries, but this is merely speculation: there are certainly many factors at play.

Table 3.6: Evidence on learning effort from the SBA survey

*Reported statistics are means, per worker hired for advertised position*

PANEL A: SBA survey 1992

Education of most recent hire	Applications received	Applicants interviewed	HR labour hours	Sample
HS dropout	9.89 (27.15)	2.61 (2.84)	4.76 (6.94)	72
HS graduate	9.91 (16.18)	4.54 (5.75)	9.31 (12.44)	415
Some college	12.31 (23.05)	4.83 (4.92)	12.16 (16.60)	290
College graduate	26.25 (54.41)	5.72 (5.65)	25.89 (43.56)	207
Post graduate	35.49 (56.54)	7.66 (8.82)	33.77 (37.79)	51

PANEL B: SBA survey 2001

Education of most recent hire	Applications received	Applicants interviewed	HR labour hours	Sample
HS dropout	3.66 (4.49)	3.03 (3.79)	11.19 (22.55)	85
HS graduate	5.30 (12.10)	3.15 (3.32)	8.10 (11.78)	415
Some college	6.21 (7.98)	4.33 (5.66)	13.15 (21.00)	182
College graduate	12.36 (23.99)	4.77 (8.71)	23.12 (37.83)	148
Post graduate	10.51 (13.03)	3.80 (3.11)	40.51 (76.89)	33

Standard errors in parentheses.

useful evidence for firms can be garnered from a pair of employers surveys in 1992 and 2001 funded by the Small Business Administration (SBA) and conducted by the Survey Research Center at the University of Kentucky<sup>35</sup> (see Berger et al., 2001). Of interest to this chapter, respondents were asked a number of questions related to the application process of their most recent hire, together with that hire's highest qualification. I divide the sample into six education categories; and for the sample of respondents within each category, I report three measures of learning effort in Table 3.6: (1) the mean number of applications received, per worker hired for the advertised position; (2) the mean number of applicants interviewed, per worker hired; and (3) the mean number of human resource hours invested in the application process, per worker hired.

I report results for the 1992 and 2001 surveys separately in Panels A and B. It should be noted that the results do vary across surveys. In particular, the number of applications received per hire was significantly larger in the earlier survey, with a sample average of 15.1 compared to 6.7 in 2001; though the reported standard errors for this variable were much larger in 1992. In any case, across both surveys, it is clear that the three firm learning effort indicators are almost always increasing in education. In 1992, the effects are very large and monotonic across education groups. On average, firms receive 35 applications, conduct 8 interviews and spend 34 human resource hours per hire at postgraduate level; but these numbers are just 10, 3 and 5 at high school dropout level. The story is qualitatively similar in 2001, but firms in that sample do receive more applications and conduct more interviews at college graduate than post graduate level; though the education effects on human resource hours are monotonic.

Note that, despite high skilled markets being much tighter (as demonstrated in Figure 3.9), there are still many more applicants per position. It follows that higher skilled workers must individually be applying to many more jobs than the low skilled: that is, in higher skilled markets, the workers appear to be investing more learning effort - and not just the firms. This is consistent with the model's predictions.

The model also predicts that firms apply learning effort more broadly geographically when recruiting high skilled workers. The SBA survey does not request this sort of information. But, there is some useful evidence in an annual survey of recruitment conducted by the Chartered Institute of Personnel and Development (CIPD) in the UK. In particular, the annual report of 2004 presents data on firms' advertising strategies by occupational rank. In the CIPD sample, 61% of firms recruiting managers and professionals post job ads in national newspapers and 67% post ads in the trade press; this compares to just 8% and 6% respectively for firms recruiting manual or craft workers. In contrast, manual/craft recruiters are more likely to post ads in local newspapers: 78% compared to 48% for manager/professional recruiters.

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<sup>35</sup>In both 1992 and 2001, the investigators contacted a nationally representative sample of establishments. In 1992, 1,288 establishments completed the survey (with a response rate of 55.9% to 60.6%, depending on the method used); and in 2001, there were 1,024 completions with a response rate of 47.1% to 48.1%.

### 3.8 Conclusion

Low skilled workers are less mobile geographically; and the evidence suggests they are less likely to leave cities following local declines in labour demand. It is often claimed that prohibitive migration costs can explain these facts. But, in this chapter, I have presented new evidence that skill differences in cross-city mobility can be better understood in the context of job matching frictions.

To this end, I have presented new evidence on migration rates, disaggregated by primary motivation, from the CPS. While higher skilled workers are more likely to migrate across counties and states for the sake of a *specific* job match, they are *less* likely to move for other reasons - whether due to family, housing, amenities or to search for work. This is true whether I study differences across education groups, or differences by occupational skill level *within* education groups.

Using a multi-city matching model, I argue that matching frictions in skilled markets are overcome through large job creation and investment in learning effort by both firms and workers; and I present evidence to that end on vacancy rates and recruitment strategies. These market features are driven by the large job surpluses in these markets, which in turn originate from large average productivity and dispersion in match quality. This superior geographical integration of skilled markets facilitates swifter exits from declining cities - with workers searching for jobs in multiple cities simultaneously, rather than making speculative moves in hope of work. And, the large surpluses can explain why skilled workers are less willing to give up a job match for family, housing or amenity reasons.

Steep cross-city search costs in low skilled markets (whose importance has been emphasised by Gregg et al., 2004, and Malamud and Wozniak, 2012) are an endogenous *outcome* of the model. But, these are one just one manifestation of the paltry job surpluses in these markets. Given these substantial obstacles to mobility, it is not clear whether policy interventions (such as relocation vouchers) can have a large effect. Ultimately, any successful policy will, to some extent, have to address the root causes of the problem, namely loose markets and limited investment in learning by both workers and firms.

While I have focused on migration in this chapter, the model may also yield interesting insights for commuting decisions. In particular, because of the larger job surpluses, higher skilled workers are likely to accept jobs which necessitate a longer commute. On the other hand, in a monocentric city, they may be able to better afford the high rents associated with proximity to the central business district. Indeed, I show above that high skilled workers are more likely to change residence to reduce their commute to work. These countervailing effects and their implications for residential segregation are worthy of further study.

# Chapter 4

## Descriptive Facts on Local Joblessness in the UK and US

### 4.1 Introduction

The incidence of joblessness and poverty varies greatly across regions and cities. Much of this is clearly driven by the characteristics of the local population. But, there is a widespread perception that, even controlling for these differences, location matters for economic opportunity. Of course, such disparities in welfare can only exist to the extent that individuals are immobile geographically. As a result, policymakers have always paid close attention to the “mobility” of the workforce.

In this field, cross-Atlantic comparisons have traditionally generated much interest. Regional disparities in participation and unemployment rates tend to be larger in Europe than the US; and a popular explanation is that migration across regions is more common in the US (OECD, 2005). Blanchard and Katz (1992) find that inter-state migration is very responsive to local demand shocks in the US, and state-level unemployment shocks usually dissipate after just five to seven years. Also, Bentivogli and Pagano (1999) find that net population flows between regions are significantly more responsive to local shocks in the US compared to Europe.

In this study, I document some stark new descriptive facts on local joblessness, based on cross-city comparisons within the US and UK. I proxy the productivity of labour in a city by its average wage. In the US, more productive cities are larger and more densely populated - though unemployment rates are unrelated to wages. Participation rates were higher in productive cities in 2010, though this seems to have been a temporary result of the recession: it was not the case in 2000. In contrast, the UK’s most productive cities do not generally have larger populations - though they have had significantly (and persistently) lower unemployment and inactivity rates since the 1980s. (London is an important outlier - with both high wages and high unemployment, and I include a separate analysis on the city below.) These effects cannot be explained by local differences in observable worker characteristics.

All this confirms that employment in American cities responds relatively quickly to shocks. A notorious example is Detroit, which bore the brunt of the decline in the American automobile industry: the population has fallen by more than half since the 1950 peak of 2m, and 23% of homes are unoccupied<sup>1</sup>. But, in British cities, local downturns are more likely to result in persistent unemployment than large out-migration and a smaller population. The variation in joblessness is largely due to the low skilled (consistent with evidence from Gregg et al., 2004) and men rather than women. Also, it is largely the *young* who are responsible for the high *unemployment* in low-demand cities, and *older* workers are responsible for low *participation*.

Beyond documenting key facts, I speculate on possible explanations for these patterns. Given that these shocks have persisted in the UK for several decades, it seems unsatisfactory to simply explain these patterns by a relatively “immobile” British workforce. An important factor may be differences between the two countries’ welfare regimes. This puts a geographical slant on Ljungqvist and Sargent (1998). They argue that unemployment was more persistent in Europe than the US after the 1980s recession, because generous benefits discouraged job search. And in the context of this chapter, it may be that generous benefits in the UK have discouraged workers from searching for jobs *outside their home city*.

Table 4.1 compares net replacement rates (after tax) for various scenarios in the US<sup>2</sup> and UK, relative to an individual earning 67% of the average wage.<sup>3</sup> Unemployment benefits are in fact more generous in the US (see Panel A), but the UK gives considerably more in terms of means-tested social assistance (Panel B). As a result, at the initial stage of unemployment, net replacement rates are comparable for the two countries for means-tested individuals: for example, these are 54% for a single person in the UK and 60% in the US.

But, once unemployment insurance has expired<sup>4</sup>, the British system performs far better at alleviating poverty for those with social assistance. In the US, the net replacement rate is just 10% for a single person, and 40% for a lone parent with 2 children (Panel B). For the UK, the numbers are 54% and 72% respectively. It is worth noting that British replacement rates are even higher for low income areas, given that most welfare payments are uniform geographically (with the exception of housing benefit).

Much of the difference between the countries is accounted for by housing benefit in the UK (OECD, 2007), which covers a household for the full rental cost of suitably sized accommodation, with allowances reflecting local prices (see Jin et al., 2010). The US does provide housing vouchers to impoverished households, but the supply of vouchers is limited and subject to long waiting lists: take-up of eligible households is just 20% (Committee on Ways and Means, 1998; referenced in Brewer, 2001; these vouchers are not accounted for in the statistics above). Also, the numbers above do not account for benefits in kind; and in

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<sup>1</sup>Census Bureau, 2010

<sup>2</sup>Benefit systems vary across the US; all US data in the table relate to Michigan, whose system is typical of the country as a whole.

<sup>3</sup>All information in the table is taken from the OECD, <http://www.oecd.org/els/benefitsandwagesoecdindicators.htm>. An accompanying analysis can be found in OECD (2007).

<sup>4</sup>In the US, benefit duration used to be limited to 26 weeks at the most, though this has been extended to 99 weeks in response to the recent recession. The UK allows for 12 months over the course of two years.



Table 4.1: Net replacement rates (%) in UK and US, relative to 67% of average wage (2010)

PANEL A: WITHOUT SOCIAL ASSISTANCE *				
	Single-person household		Lone parent, 2 children <sup>oo</sup>	
	With UB <sup>o</sup>	Without	With UB <sup>o</sup>	Without
UK	19	0	45	0
US	60	0	53	0

PANEL B: WITH SOCIAL ASSISTANCE **				
	Single-person household		Lone parent, 2 children <sup>oo</sup>	
	With UB <sup>o</sup>	Without	With UB <sup>o</sup>	Without
UK	54	54	72	72
US	60	10	60	40

PANEL C: DURATION OF UNEMPLOYMENT BENEFIT	
UK	12 months over the course of 2 years
US	14-26 weeks (2007), up to 99 weeks (2009)

This table lists net replacement rates (percent) for various scenarios in the UK and US, relative to an individual earning 67 percent of the average wage. All rates are after tax and relate to a 40-year-old with a long and uninterrupted employment history. Benefit systems vary across the US; all US data in this table relate to Michigan, whose system is typical of the country as a whole. \* Panel A: No social assistance "top-ups" or cash housing benefits are assumed to be available in either the in-work or out-of-work situation. \*\* Panel B: Social assistance and other means-tested benefits are assumed to be available subject to relevant income conditions. Housing costs are assumed equal to 20 percent of the average wage. Where receipt of social assistance or other minimum-income benefits is subject to activity tests (such as active job-search or being "available" for work), these requirements are assumed to be met. <sup>o</sup> Unemployment benefit. Any income taxes payable on unemployment benefits are determined in relation to annualised benefit values (i.e. monthly values multiplied by 12) even if the maximum benefit duration is shorter than 12 months. <sup>oo</sup> Children are aged four and six, and neither child-care benefits nor childcare costs are considered. All information in this table is taken from the OECD Benefits and Wages program, <http://www.oecd.org/els/benefitsandwagesoecdindicators.htm>.

particular, public health coverage, which is comprehensive in the UK.

Still, the remarkable persistence of local employment differentials in the UK cannot all be ascribed to the welfare system. If there are more jobs elsewhere, then workers should eventually move away, even if this takes some time. I speculate that the missing ingredient is the adjustment of local housing markets. Local prices are so low in the low-demand cities that *real* wages (or local spending power) are actually *higher* than elsewhere; this can be appreciated from local real wage estimates from Gibbons et al. (2011). These high real wages, available on an eventual return to employment, discourage workers from moving elsewhere. But, how could prices have dropped more than wages in these areas? One possible answer is the durability of housing: housing stocks do not disappear, even if a city is in decline (Glaeser and Gyourko, 2005). A decline of local industries can destroy much of the stock of human capital (as skills become valueless). But, housing capital is largely unaffected, and this relative excess of housing can cause prices to fall quicker than wages.

The low housing costs discourages unemployed workers from moving elsewhere. But, for high unemployment to persist for many years, there must also be some downward stickiness of wages that prevents the local labour market from clearing. There are a number of possible reasons for such stickiness. Firstly, early retirement from the labour force is likely to reduce the pressure on wages through a hysteresis effect (Blanchard and Summers, 1986). Secondly, welfare payments in the UK are largely insensitive to local living costs (with the important exception of housing benefit which provides financial support for rental payments), so they form a more rigid floor for reservation wages. Perhaps more importantly, wage policies also tend to be uniform nationally. In particular, the UK's National Minimum Wage has a larger "bite" in low-wage areas, and Dolton et al. (2008) show that the impact on the wage structure has been larger in these places. Public sector scales are also largely invariant across regions (with the exception of London bonuses), so they are likely to impose a larger constraint on private sector wage setting in low-wage areas. Indeed, using geographical variation, Faggio and Overman (2013) find that public sector employment crowds out local jobs in tradable sectors.

The fact that the employment patterns pertain largely to the low skilled is consistent with the story from Chapter 3. In particular, better educated workers pay less attention to local *average* wages and *average* housing costs when choosing where to live: they are more concerned with finding the best possible *individual* job match. As a result they search for jobs over longer distances, so local pockets of unemployment and inactivity are less likely to form.

These ideas contribute to a growing literature on the impact of the housing market on regional job mismatch. In particular, Sterk (2010) links the housing crash in the US with the breakdown of the Beveridge Curve in 2009. A negative shock to house prices reduces home equity levels, and this makes it harder to provide the down payment for a new mortgage. Therefore, he argues that more unemployed homeowners are forced to reject job offers outside their home city. This results in larger unemployment, at any given level of vacancies. Karahan and Rhee (2013) provide MSA-level empirical evidence for this phenomenon.

Returning to the evidence, London is an important outlier in the relationships I described above. While wages are extremely high, unemployment and inactivity rates are comparable to Northern areas. In fact, in 2005 (before the recent recession), London had a higher unemployment rate than any other region. The origin of much of this appears to be the recession of the early 1990s, which hit the South the hardest. But, in fact, unemployment had already begun rise in London relative to elsewhere in the 1980s. As time has progressed, it has become clearer that London’s distinctiveness is not a mere temporary blip, but rather, appears to be a more structural phenomenon. However, there has been little analysis in the literature on this subject.

The coexistence of high wages and high unemployment in London is suggestive of persistent shocks to labour *supply*. One possible candidate is immigration. Since the 1980s, London has experienced distinctively large inflows of foreign migrants, with annual net foreign inflows to the city reaching 1% of the local population in the early 2000s. Evidence from Dustmann et al. (2013) suggests the wages of low skilled Londoners have been adversely affected by migration. Also, foreign investment and possibly immigration have contributed to rapid house price growth in the city (The Economist, 2012). These factors can help explain persistent net out-migration from London to the rest of the country (of relatively older and lower skilled workers) of a similar magnitude as the local migrant inflows. For those who remained in the city, it is possible that depressed low skilled wages and high prices have reduced the returns to work in the face of the welfare alternative. Of particular importance is housing benefit, which compensates families for the *local* cost of housing.

In the following section, I discuss the data I use for the UK and US. Section 4.3 documents the key cross-city labour market patterns in each country in 2010. In Section 4.4, I study the historical evolution of the patterns currently observed in the UK. Section 4.5 provides some evidence on the role of local house prices in cementing regional employment disparities in the UK, and in Section 4.6, I document related features of local housing markets. I look at the case of London in more detail in Section 4.7, and Section 4.8 concludes.

## 4.2 Data

### UK

In the British data, I identify cities with travel-to-work areas (TTWAs), as defined by the ONS in 2001. There are 232 TTWAs (excluding Northern Ireland), covering the full landmass of the country. They are intended to represent labour market zones: their boundaries have been drawn to ensure that 75% of the residents of each TTWA work in the same one, and that 75% of those who work in each TTWA live in the same one.<sup>5</sup>

However, most of the local data I use is based on Local Authority Districts and Unitary Authorities (LAs), of which there are 380. In many cases, the boundaries of TTWAs and LAs do not match exactly. Therefore, I reconstruct the TTWAs into entities that are fully

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<sup>5</sup><http://www.ons.gov.uk/ons/guide-method/geography/beginner-s-guide/other/travel-to-work-areas/index.html>

identifiable by the LAs, based on geographical look-up tables from the Cathie Marsh Centre for Census and Survey Research<sup>6</sup>. Specifically, I assign each LA to the TTWA which occupies the largest share of its area. As a result of this exercise, I dropped many of the smaller TTWAs, leaving me with 178.

Average house prices for LAs are based on Land Registry data, and compiled by the Department for Communities and Local Government (DCLG) for England and Wales<sup>7</sup> and by the Registry for Scotland<sup>8</sup>. The DCLG also provides local-level information on planning refusal rates for England, which I use as a proxy for constraints on local housing supply. This is the share of planning decisions for residential developments (1 dwelling or more) that were negative. A number of studies on housing supply (e.g. Hilber and Vermeulen, 2010) have made use of this data.

My local weekly earnings data is based on the Annual Survey of Hours and Earnings (ASHE), extracted from Nomis<sup>9</sup>. For my TTWA-level unemployment data, I use ONS model-based estimates also from Nomis. These are based largely on the Annual Population Survey<sup>10</sup> (APS) of 2010, together with local information on claimants counts to improve precision for small areas. To study employment patterns within local areas, I use the APS microdata itself. I also use the APS to construct other TTWA-level housing and labour market indicators, including housing benefit take-up and housing tenure.

A variable that is particularly important to this chapter is the local real wage: the spending power of local workers, given geographical variation in house prices. These have been calculated by Gibbons et al. (2011) for 121 of the TTWAs in my dataset. Their motivation was to create a proxy for local amenity values, based on the assumption that, in equilibrium, workers should be on average indifferent between locations; given this, amenity values should be inversely related to the local real wage. Their estimates are adjusted for taxes and also labour and housing quality: they control for individual characteristics in the calculation of wages (as well as individual fixed effects, using longitudinal earnings data between 1998 and 2007); and similarly, for housing characteristics in the calculation of local housing costs. They present a number of different “quality of life” estimates; the local real wage is the negative of these. I use the estimates predicated on the assumption that households in different cities spend the same fraction of earnings on housing (based on Cobb-Douglas utility); these are the most comparable with the available estimates for US cities (Albouy, 2012)

I also study historical patterns in local employment. Unfortunately, I currently only have access to historical labour and housing market data at TTWA-level for the last ten years or so. To study local patterns further back in time, I use the statistical regions of the UK: there are 12 of these, or 11 excluding Northern Ireland. My regional non-employment series

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<sup>6</sup><http://www.ccsr.ac.uk/research/lookup.htm>

<sup>7</sup><http://www.communities.gov.uk/housing/housingresearch/housingstatistics/>

<sup>8</sup>[http://www.ros.gov.uk/public/news/quarterly\\_statistics\\_time\\_series.html](http://www.ros.gov.uk/public/news/quarterly_statistics_time_series.html)

<sup>9</sup><http://www.nomisweb.co.uk/>

<sup>10</sup>The APS is based on the Labour Force Survey (LFS) samples, with the addition of booster samples to aid estimation of local area statistics.

are estimated from LFS cross-sections<sup>11</sup> since 1975.

In Section 4.7, I make use of regional-specific migration data, collated by the ONS in the Regional Trends reports since 2000<sup>12</sup>. For each region (and over a number of years since 1981), the ONS has estimated in/out-migration to/from abroad and in/out-migration from other parts of the UK. The international data is based on the International Passenger Survey (IPS), which samples passengers passing through air and sea terminals both in and out of the country; one question asks for the respondents' region of origin or destination in the UK. The inter-regional data is based on administrative records of patients re-registering with NHS doctors elsewhere in the country.

I also look more closely at the characteristics of migrants entering and leaving London. However, the APS samples are too small for this kind of local analysis, given the relatively small number of migrants. Instead, I use the Small Area Microdata sample from the UK census, which is a 5% sample with detailed geographical information. In terms of migration, the census asked respondents for their area of residence both currently and 12 months previously. I also use the census data for information on household density (persons per room), which cannot be constructed from the APS.

Finally, I estimate local population densities using population data from Nomis and local land area data from UK Standard Area Measurements at the ONS<sup>13</sup>.

## United States

I identify the cities in my model with the Metropolitan Statistical Areas (MSAs) of the US. They consist of a central densely populated city, together with surrounding areas which have close economic ties to the centre. Like the TTWAs, they can be understood as representing local labour markets. All city-level variables are estimated from the IPUMS American Community Survey (ACS) of 2010, a nationally representative 2% sample organised by Ruggles et al. (2010a). Information on land area is included in the IPUMS 5% census extract of 2000, which I use to estimate population density.

I take estimates for local real wages from Albouy (2012): the negative of his “quality of life” indicators. These are calculated using similar assumptions to Gibbons et al. (2011) and adjusted for both local labour and housing quality.

## 4.3 Labour market

### Stylised facts

Figure 4.1 gives the key stylised facts that motivate this project. The general picture is as follows: in the UK, local productivity manifests itself more in terms of employment *rates*;

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<sup>11</sup>The LFS was conducted on a quarterly basis since 1992, with a limited five-quarter longitudinal aspect. In these cases, my annual sample consists of the first observation of each individual in the quarterly samples.

<sup>12</sup><http://www.ons.gov.uk/ons/publications/all-releases.html?definition=tcm%3A77-21847>

<sup>13</sup><http://www.ons.gov.uk/ons/guide-method/geography/products/other/uk-standard-area-measurements-sam-/index.html>

and in the US, more in terms of local employment *density* or *size*.

Each graph is a plot, across cities, of a local variable of interest on the log mean weekly full-time wage, my proxy for local labour productivity. The left column is based on the TTWAs of the UK, and the right column reproduces these same graphs for the MSAs of the US.

The first row of graphs looks at unemployment rates. In the UK, more productive cities (with the exception of London, to which I return later) have significantly lower unemployment rates. In 2010, these rates range from, on average, 5% in the most productive cities to 10% in the least. Excluding London, the R squared is over 30%. In the US, there is also a negative relationship, but it is significantly weaker: the estimated coefficient of the effect is about a fifth of the UK's, and the R squared is just 2%.

A similar picture exists for inactivity rates (for all individuals aged 16-64), though the difference across countries is not as stark. For the UK, excluding London, these range from under 20% in the most productive cities to around 25% in the least. In the US, the estimated effect of wages is about half of that in the UK. This is not because of cross-country differences in local inactivity differentials, but rather because the range of local wages in the US is larger. Much of this correlation in the US is simply a result of the recent recession: in 2000, the R squared is only 2%. For the UK, as I show below, these effects have been very persistent over time.

The last row looks at log local population density. In the UK, density is totally uncorrelated with local wages, but in the US, there is a strong positive correlation between the two (with an R squared of 37%). The most productive cities are about three times the size of the least. The patterns for log population size, not reported here, are very similar.

Before moving on, I briefly discuss the case of London. Despite its high average wage, London has relatively high unemployment and inactivity rates. One explanation is based on London's size: it is much larger than the other British cities, and people commute very long distances. So, decomposing London into its constituent pieces may shed some light.

In the first graph of Figure 4.2, I plot local unemployment rates against log wages, across British TTWAs. But, this time, I disaggregate London into its boroughs and the surrounding LAs in its TTWA. Note that the wage variable reflects the wage of local *residents* rather than employees. It is clear that *almost all* London's boroughs suffer from relatively high unemployment given their local wage. Interestingly, unemployment rates in the city seem to reflect the full range of rates across the country: unemployment in Richmond is under 6%, but over 13% in Newham. I also repeat the same exercise in the second graph of Figure 4.2, but this time put log house prices on the x-axis. The same pattern emerges: given local prices, almost all of London's boroughs have very high unemployment rates. In Section 4.7, I propose an explanation for these facts based on recent migration trends.

## Patterns across demographic groups

To better understand these phenomena, it is important to check exactly which demographic groups are responsible for them. Also, there may be concern that the observed effects

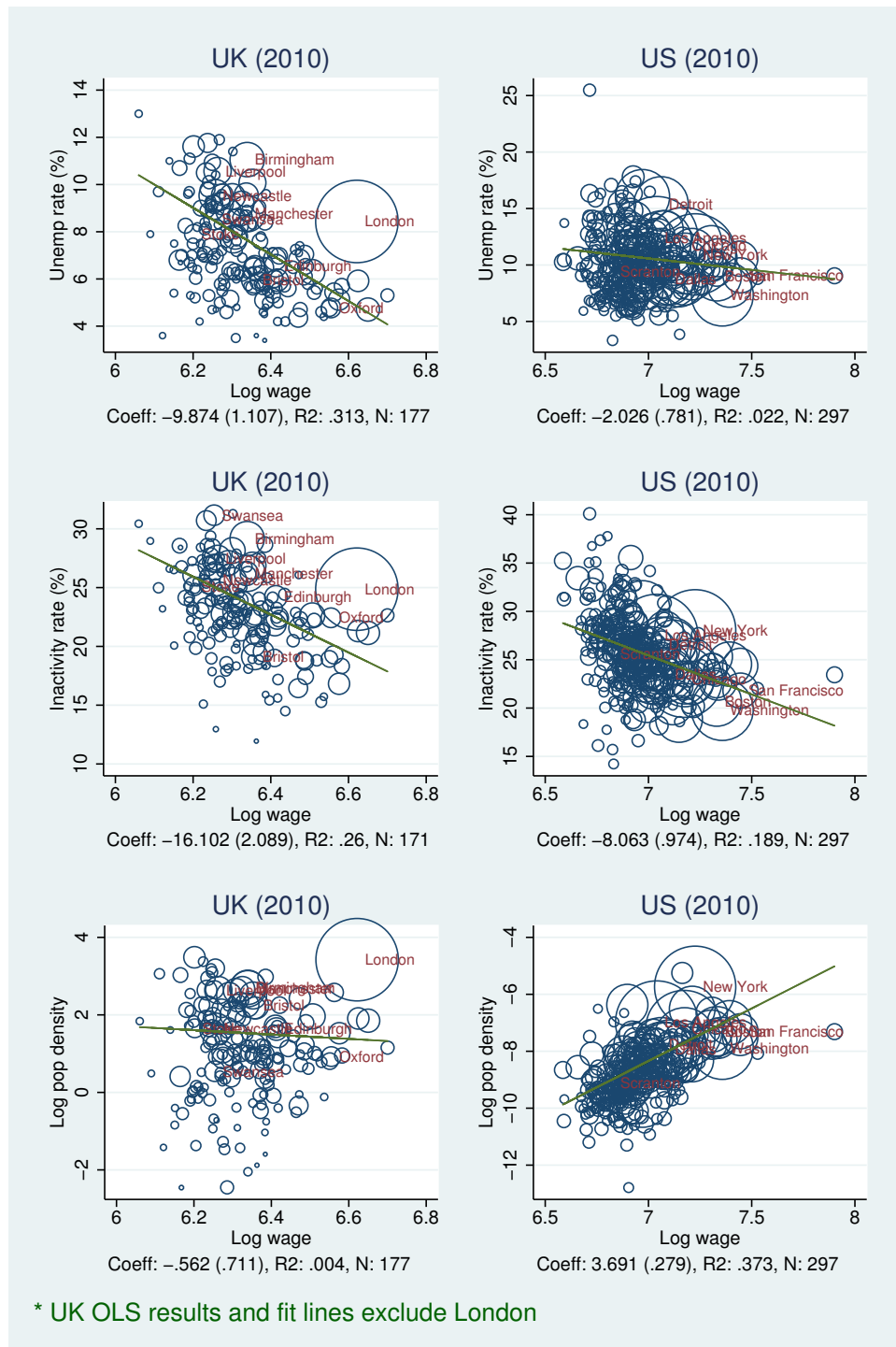


Figure 4.1: Cross-city relationships for UK and US: labour market

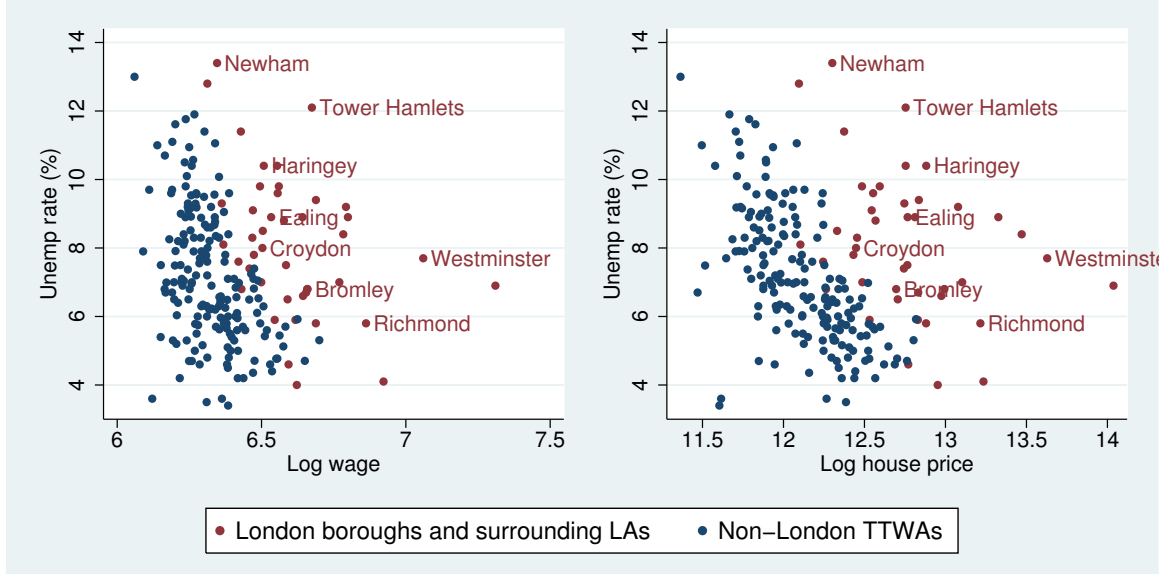


Figure 4.2: Disaggregation of London

are merely due to cross-city differences in local demographic compositions. Controlling for individual characteristics can address this criticism.

Using microdata from the UK (the APS of 2010) and the US (the ACS of 2010), I regress individual-level unemployment and inactivity against the log local wage, separately for different demographic groups. In the UK, I exclude London from the sample. In this version, I have used OLS linear probability models rather than probit because of temporary technical problems. The estimating model is:

$$y_{ic} = \beta_0 + \beta_1 w_c + X_{ic}\gamma + \varepsilon_{ic} \quad (4.1)$$

where  $i$  denotes individual, and  $c$  is city.  $y_{ic}$  is a binary variable, taking 1 for unemployment or inactivity;  $w_c$  is the log local mean weekly full-time wage; and  $X_{ic}$  is a vector of detailed individual characteristics (quartic in age, interacted with education categories; ethnicity and immigrant indicators). The errors  $\varepsilon_{ic}$  are clustered by city.

Table 4.2 reports the results for unemployment, based on the sample of labour market participants aged 16-64. The first two columns of results are for the UK, and the final two for the US. For each country, the first column restricts the sample to men and the second column to women. The reported numbers are estimates of  $\beta_1$ , the coefficient on wages, with its standard error in parentheses below.

The first row is an unconditional regression with no individual controls. Consistently with Figure 4.1, the effect of log wages is about five times as large for the UK compared to the US for men. Interestingly, for the UK, the effect for men is more than double that for women. When individual controls are included in the second row, the UK estimates fall by about 30%, but they remain strong and large. In the US, including controls causes the estimates to go to zero.



Table 4.2: Effect of local wages on unemployment for UK and US (2010)

Sample	Indiv controls?	UK		US	
		Men	Women	Men	Women
All	No	-0.135*** (0.017)	-0.062*** (0.015)	-0.031*** (0.011)	-0.013 (0.010)
All	Yes	-0.094*** (0.015)	-0.043*** (0.013)	0.011 (0.013)	0.008 (0.011)
Age: 16-21	Yes	-0.237*** (0.070)	-0.216*** (0.072)	0.012 (0.026)	0.004 (0.021)
Age: 22-29	Yes	-0.099*** (0.037)	-0.002 (0.036)	0.019 (0.014)	-0.005 (0.014)
Age: 30-39	Yes	-0.112*** (0.028)	-0.044** (0.020)	-0.002 (0.013)	-0.005 (0.011)
Age: 40-49	Yes	-0.092*** (0.016)	-0.045*** (0.016)	0.008 (0.014)	0.020 (0.013)
Age: 50-64	Yes	-0.040** (0.019)	-0.018 (0.015)	0.018 (0.014)	0.016 (0.011)
Edu: < A-level <sup>o</sup>	Yes	-0.189*** (0.032)	-0.098** (0.039)	-0.038* (0.021)	-0.051 (0.027)
Edu: A-level <sup>o</sup>	Yes	-0.065* (0.036)	-0.098*** (0.025)	0.014 (0.016)	-0.001 (0.015)
Edu: Tertiary	Yes	-0.073*** (0.014)	-0.004 (0.014)	0.022** (0.011)	0.020** (0.009)
Homeowners	Yes	-0.046*** (0.014)	-0.021** (0.010)	0.019 (0.014)	0.013 (0.010)
Renters	Yes	-0.253*** (0.034)	-0.130*** (0.040)	-0.006 (0.013)	-0.008 (0.013)

This table reports coefficients from individual-level OLS regressions of unemployment on local mean wage (weekly, full time), for both the UK (excluding London) and US. The UK microdata is the APS of 2010, and the local wage data is from the ASHE (extracted from Nomis). All US data is from the ACS of 2010. In each case, the sample is restricted to labour market participants aged 16-64. The full UK sample is 130,000 observations, spanning 171 TTWAs; and the full US sample is 1.1m observations, spanning 297 MSAs. In all rows but the first, I control for individual characteristics (quartic in age, interacted with education categories; ethnicity and immigrant indicators). Coefficients are estimated separately for men and women, and for a range of sample groups. All regressions are clustered by city, SEs in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. <sup>o</sup> A-level in UK, high school diploma in US.

The remaining rows report the effects for various age and education groups, as well as separately for homeowners and renters. In each case, I have included the full range of individual controls. For the US, there is little evidence of significance for any subgroup - though there is a strange positive result for those with tertiary education. For the UK, a number of interesting patterns emerge. The effect is much larger for younger workers, especially for those aged 16-21: their coefficient is six times as large as for the over 50s. The effect is also much larger for the low skilled: the coefficient for those without A-levels (or equivalent) is more than twice as large as those with higher education. And finally, the effect is largely driven by renters (though these do tend to be younger and lower skilled anyway).

In Table 4.3, I repeat the same exercise for inactivity. For the UK, controlling for individual characteristics in the second row again removes about 30% of the effect for men, though it remains large and significant. In the US, including individual controls makes the coefficient statistically insignificant for men, though not for women.

As before, for the UK, the effect is much larger for men: about five times as large as for women for the conditional full sample effect (in row 2). And again, the effect is larger for the low skilled; the coefficients are very similar to those for unemployment. However, the age effects are reversed. The coefficient is now insignificant for those under 30, and reaches -0.22 for the over 50s. Interestingly, this is exactly the same number as for the 16-21s for unemployment (Table 4.2). This may have a life-cycle explanation. In the UK, low skilled men are the most sensitive to regional variation. When they first enter the labour market in low-demand cities, they are frequently unemployed, with little prospect of skill accumulation. And, as a result, they may be more likely to leave the labour force early in later life.

In the US data, while the effects are sometimes negative and significant for inactivity, they are much smaller than in the UK, and there are fewer clear patterns. For some groups, the effect is larger for women, and for others, for men. The effects do tend to be larger for older workers (strangely, there is a strong positive effect of wages for men 16-21s). And, for education groups, the effects are larger for low skilled women, though it is the reverse for men. As I explained above, these effects are specific to 2010 only; they are hardly present in 2000. So, differences in patterns across subgroups may simply be a by-product of the selective impact of the recession - as opposed to a longer term life-cycle phenomenon.

Note that these differences between the UK and US are not the result of differences in sampling error. In fact, the standard errors for the US tend to be smaller. This is unsurprising: the US data has over 100 more cities and a much larger individual sample (1.4m compared to 170,000) than the UK.

## 4.4 Changes over time

The UK relationship between local employment and productivity, illustrated above, was not always so strong. But, once it appeared, it has proved remarkably persistent. In this section, I discuss its historical evolution.

Data has not been available for TTWAs for many years, so I look at regions instead. Also, since I cannot currently access high-quality regional wage data before 1990, I study

Table 4.3: Effect of local wages on inactivity for UK and US (2010)

Sample	Indiv controls?	UK		US	
		Men	Women	Men	Women
All	No	-0.190*** (0.022)	-0.134*** (0.026)	-0.073*** (0.017)	-0.065*** (0.022)
All	Yes	-0.125*** (0.018)	-0.032* (0.017)	-0.023 (0.016)	-0.037** (0.016)
Age: 16-21	Yes	-0.081 (0.066)	-0.066 (0.082)	0.115*** (0.042)	0.050 (0.044)
Age: 22-29	Yes	-0.066 (0.042)	-0.079 (0.054)	0.001 (0.021)	-0.049** (0.023)
Age: 30-39	Yes	-0.061*** (0.019)	0.118*** (0.033)	-0.027*** (0.010)	-0.028 (0.018)
Age: 40-49	Yes	-0.139*** (0.022)	-0.003 (0.043)	-0.035*** (0.012)	0.006 (0.017)
Age: 50-64	Yes	-0.219*** (0.027)	-0.134*** (0.034)	-0.090*** (0.017)	-0.106*** (0.012)
Edu: < A-level <sup>o</sup>	Yes	-0.195*** (0.033)	-0.190*** (0.038)	-0.001 (0.028)	-0.053* (0.032)
Edu: A-level <sup>o</sup>	Yes	-0.126*** (0.038)	-0.095*** (0.029)	-0.019 (0.018)	-0.061*** (0.022)
Edu: Tertiary	Yes	-0.097*** (0.019)	0.065*** (0.019)	-0.029** (0.012)	-0.020 (0.013)
Homeowners	Yes	-0.066*** (0.016)	0.025 (0.021)	-0.024* (0.013)	-0.026* (0.015)
Renters	Yes	-0.267*** (0.042)	-0.178*** (0.053)	-0.027 (0.021)	-0.069*** (0.023)

This table reports coefficients from individual-level OLS regressions of inactivity on local mean wage (weekly, full time), for both the UK (excluding London) and US. The UK microdata is the APS of 2010, and the local wage data is from the ASHE (extracted from Nomis). All US data is from the ACS of 2010. In each case, the sample is restricted to all individuals aged 16-64. The full UK sample is 170,000 observations, spanning 171 TTWAs; and the full US sample is 1.4m observations, spanning 297 MSAs. In all rows but the first, I control for individual characteristics (quartic in age, interacted with education categories; ethnicity and immigrant indicators). Coefficients are estimated separately for men and women, and for a range of sample groups. All regressions are clustered by city, SEs in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. <sup>o</sup> A-level in UK, high school diploma in US.

the changing relationship between non-employment and house prices (I show below in Figure 4.4 that local wages and prices are closely correlated anyway). Specifically, I look at the non-employment rate of men aged 16-64: this measure takes into account both unemployment and non-participation.

In Figure 4.3, I plot the non-employment/price relationship across regions for a number of different years since 1975, based on LFS data. I also include OLS fit lines, but note that (as above) these are based on all regions excluding London, which appears to exhibit very different patterns.

The relationship has been negative throughout the period. But, excluding London, it was statistically insignificant in the 1970s. At the time, non-employment was quite low: about 13% on average. This changed with the recession of the early 1980: since then, non-employment has grown substantially (particularly because of declining male participation), and it now stands at about 25%. The increase in joblessness has been largely regional in character, with Northern areas and Wales suffering most.

Excluding London, the coefficient on the non-employment/price relationship soared from -11 in 1975 to -27 in 1985; this number measures the percentage point change in the non-employment rate associated with a doubling of house prices. It hovered between -27 and -30 until 1995, and has fallen to about -16 today. This recent fall is not because of contracting employment differentials, but rather because of soaring house prices in the South East.

The correlation between non-employment and house prices (excluding London) was no more than 11% in the 1970s, and between 50% and 85% since then. There is no sign of this tight relationship abating. Since the 1980s then, joblessness has remained persistently higher in less productive areas.

This set of graphs also illustrates well London's growing distinctiveness. In 1975, London resembled the rest of the South East, with high prices and non-employment of around 11%. But, already by 1979, a small gap had opened between the two regions, with London non-employment rising to 13%. After the recession of the early 1990s, London was almost on par with the North West at 28%. And these high levels have persisted until today: non-employment was 25% in 2010. But, despite all this, house price differentials between London and the rest of the country have ballooned in the last few decades: in the 1970s, London prices were about 50% higher than the North (the cheapest region), but they now almost twice as high. This combination of rising prices and unemployment in London is suggestive of a prolonged *supply* shock, a theory on which I elaborate in Section 4.7 below.

## 4.5 Role of local house prices

Why are British workers less likely to migrate in the face of local shocks, and more likely to risk unemployment or inactivity? The answer may lie in the generous welfare support in the UK (as discussed above). This allows workers to remain long-term cities despite a lack of jobs. However, this should not not be enough to cause shocks to persist for decades. I propose that local housing market equilibrium plays a role in cementing employment differentials over the longer term.

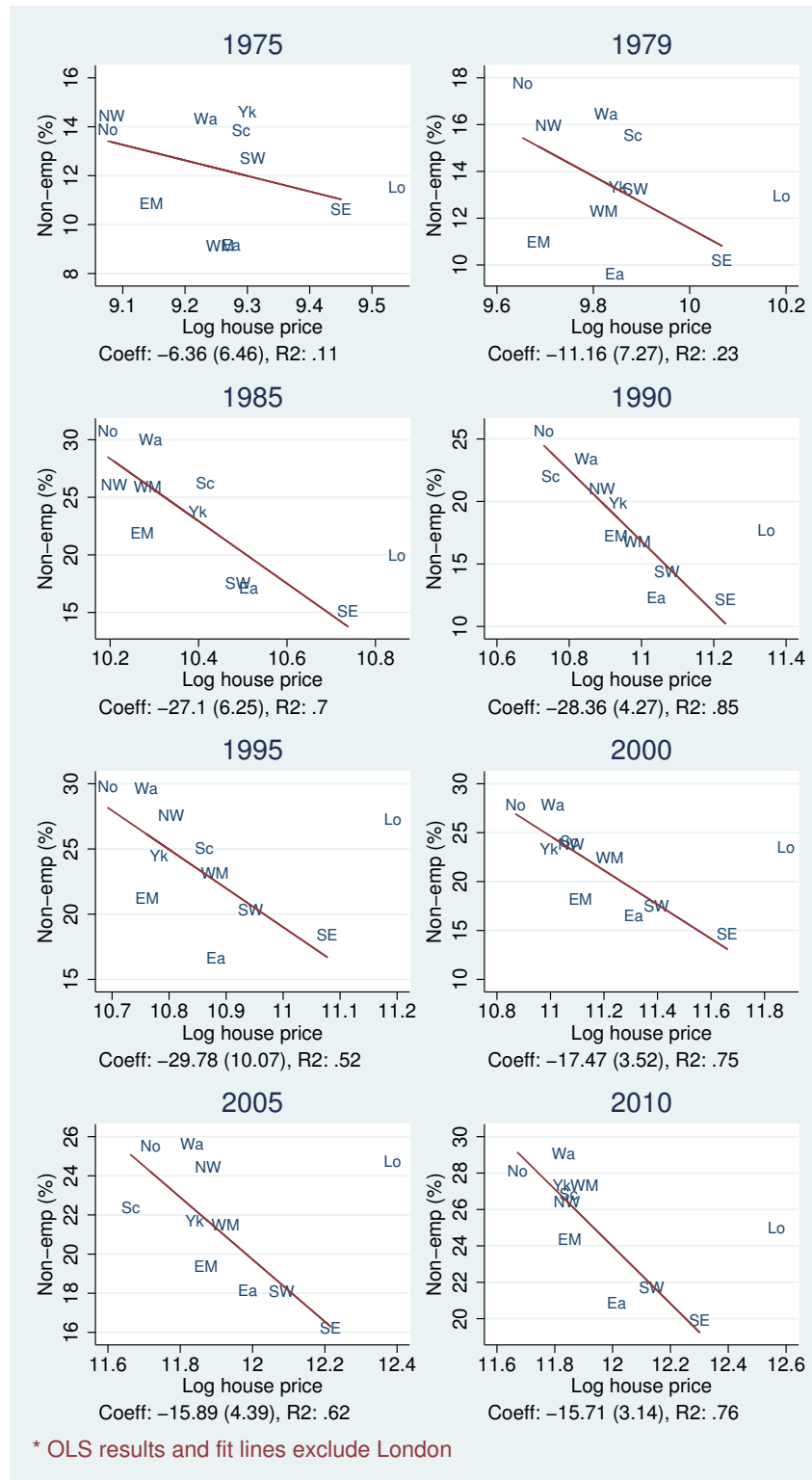


Figure 4.3: Evolving regional correlation between non-employment (men, 16-64) and house prices

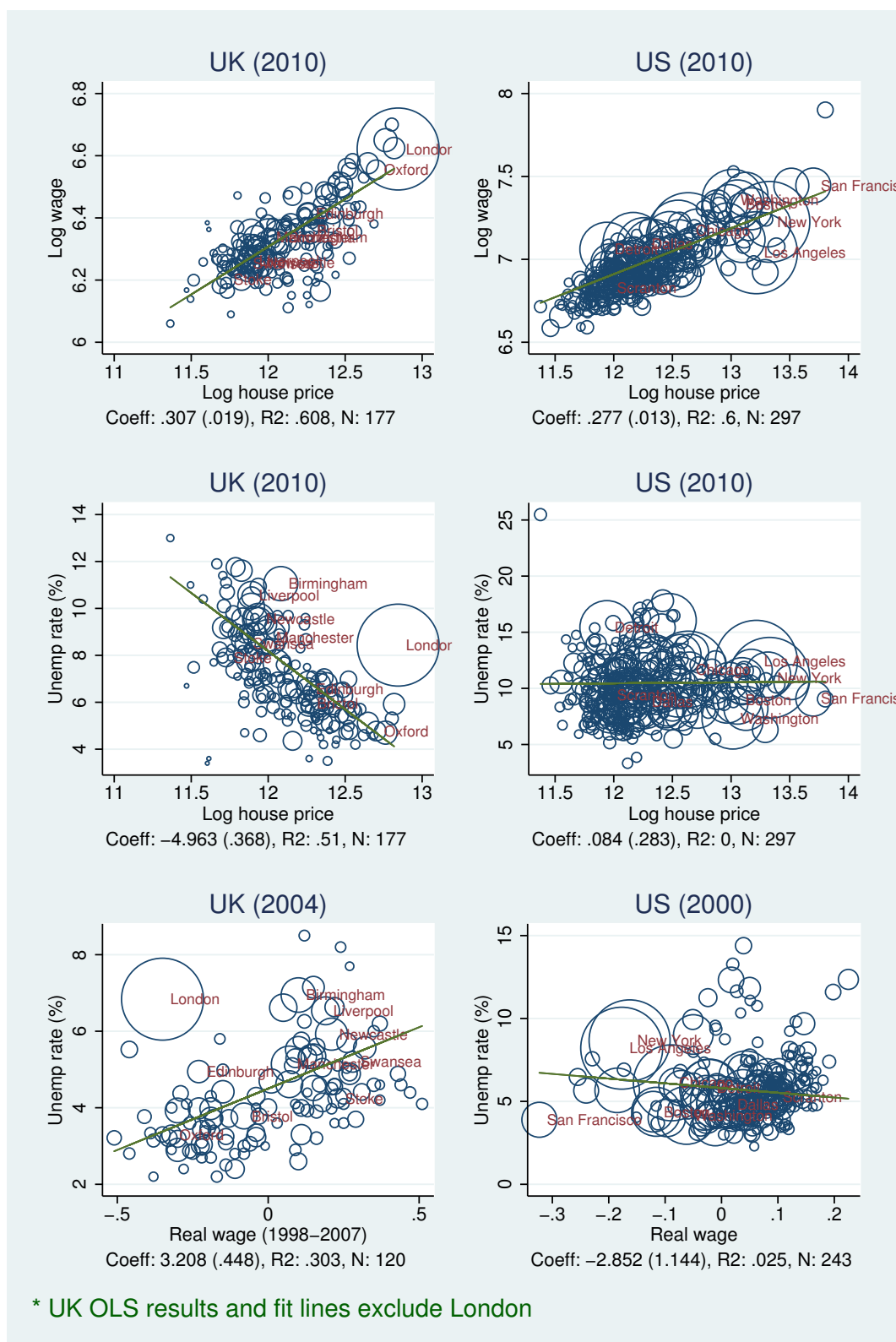


Figure 4.4: Cross-city relationships for UK and US: local prices

In Figure 4.4, I present some stylised facts on variation in local house prices, for both the UK and US. The first row shows a tight relationship between log local wages and log house prices across cities. For both countries, a doubling of prices is associated with an approximately 30% increase in local wages; and in each case, the R squared is about 60%. This can be understood in the context of the standard spatial equilibrium model. Prices will adjust to make workers indifferent between cities with different labour productivities (and wages). Assuming Cobb-Douglas preferences over housing and traded goods, the theory predicts that the regression coefficient should be equal to the share of housing consumption in total expenditure; this is indeed about 0.3 (Glaeser and Gottlieb, 2009).

Given the tight price-wage relationship, it is unsurprising that there is a strong negative correlation between local prices and unemployment in the UK (second row). Excluding London, a doubling of local prices is associated with a 5 percentage point drop in unemployment; and the R squared is 50% (which is even larger than for the unemployment-wage relationship in Figure 4.1). In the US, the unemployment-price correlation is zero; again, this should be no surprise.

Given the variation in prices and wages, what does this mean for spending power in different cities? In the final row, I plot local unemployment rates against the real wage (as estimated by Gibbons et al., 2011, for the UK; and Albouy, 2012, for the US), adjusted for both labour and housing quality.<sup>14</sup> In the US, the correlation is weak (R squared under 3%). But, in the UK, the two variables are strongly positively related (again, excluding London), with an R squared of 30%. That is: low-wage, high-unemployment cities actually have relatively high *real* wages. Effectively, local house prices are *so* much lower in these places that real wages are actually larger.

This has a spatial equilibrium interpretation: workers may be trading off the high jobless risk in low-demand cities with the higher spending power (that accrues once they find employment). This is not a new idea: Gibbons et al. (2011) discuss local job availability as an amenity and show it is correlated with their real wage estimates. But still, mechanically, what could cause prices to fall further than wages in low productivity cities? One possible explanation is inertia in local housing stocks. Glaeser and Gyourko (2005) point out that housing is durable: if a city is in decline, housing will not simply disappear. Instead, local house prices will fall steeply to maintain housing demand. In the case of the UK, we might think of the decline in Northern industries as a contraction of the *human capital* stock: local workers' skills became devalued. But, housing capital was largely unaffected (most of the UK's housing stock was built many decades before). As a result, in those areas, there is likely to be a large excess of housing capital - relative to the local human capital to be housed. The outcome was a relative fall in the price of housing capital relative to the price of human capital (the quality-adjusted wage). This reasoning is currently merely speculative, and I would need a rigorous model to test its internal and external consistency.

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<sup>14</sup>I use my earliest unemployment estimates (2004) for the UK to match the dates of the real wage estimate (1998-2007). For the US, Albouy's estimates are from 2000; so I construct unemployment rates for that year (from the 5% census extract).

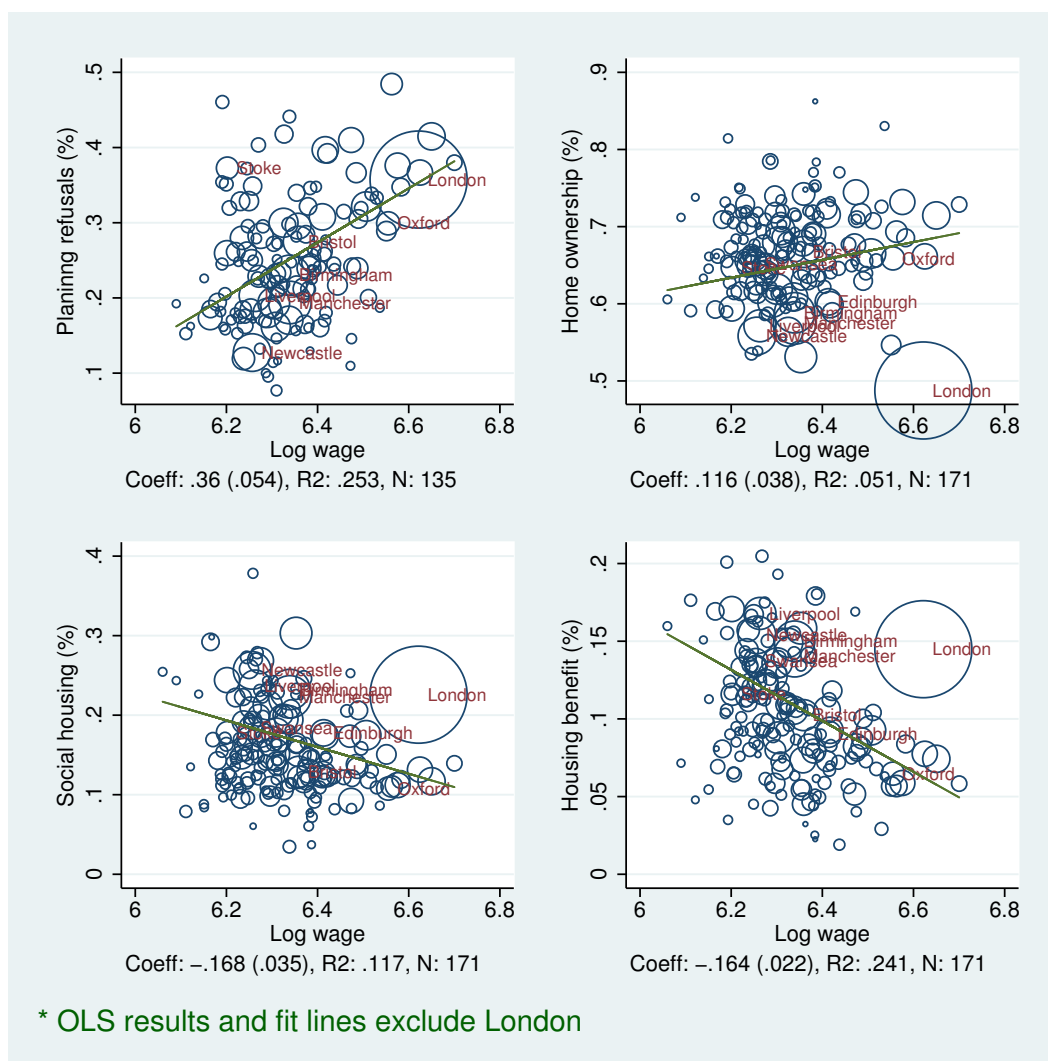


Figure 4.5: Housing market indicators across UK cities

## 4.6 Local housing market characteristics in the UK

To provide further clues on the role of housing markets, it is useful to document other features of local housing characteristics in the UK. In Figure 4.5, I plot a number of different TTWA housing indicators against log wages, my proxy for local labour productivity.

The first graph shows that the share of planning refusals (as a percentage of all decisions) is strongly increasing in the local wage, ranging from on average 15% in the least productive cities to over 30% in the most. This suggests housing supply is more constrained in those cities with the most available jobs. This presents an important obstacle to the adjustment of employment to local shocks. The US is quite different in this respect: while there are several prosperous cities that have little physical potential for growth (like San Francisco, Los Angeles or New York), there are many rapidly growing cities with almost no constraints



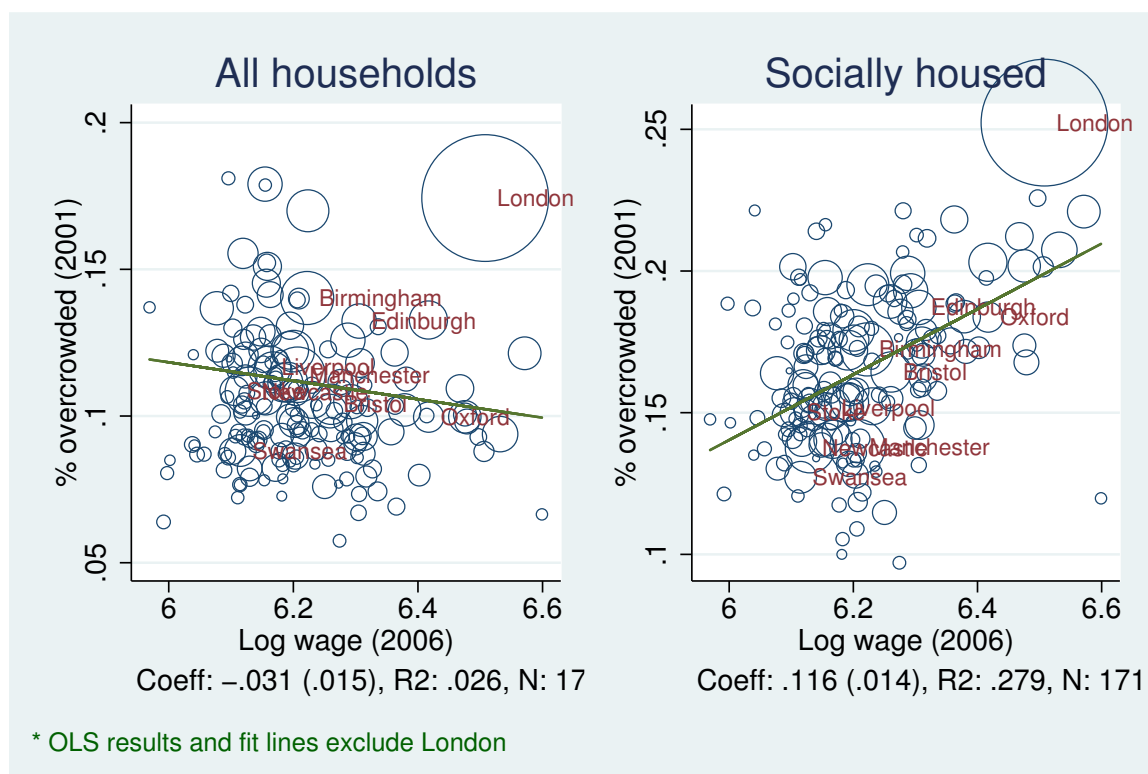


Figure 4.6: Incidence of overcrowded housing across UK cities

on expansion, particularly in the Sun Belt.

The second graph shows that homeownership rates (based on the APS of 2010) are moderately larger in high-wage cities, though the correlation is weak ( $R$  squared is 5%). London is an outlier, with ownership under 50%.

In the third graph, it can be seen that the share of households in social housing (APS 2010) is decreasing in local wages, ranging from on average 10% in the most productive cities to 20% in the least. The greater availability of social housing in low-wage cities may further encourage the unemployed to remain. Furthermore, social tenancy may well reduce the geographical mobility of its residents (Gregg et al., 2004). Again, London is an exception, with relatively large stocks of public housing given its high wage.

The last graph gives data on local take-up of housing benefit, also from the APS of 2010. Unsurprisingly, with the exception of London, this is larger in less productive cities - reaching up to 20% of all households. These high rates are demonstrative of the availability of welfare support in cities suffering economic problems.

Finally, I consider local differences in the quality of housing. At the lower end of the income scale, an important problem is overcrowded housing. One standard benchmark for overcrowding is 0.75 persons per room. In the first graph of Figure 4.6, I plot the incidence of overcrowding in cities against the local wage. The relationship is slightly negative, but very weak - with an  $R$  squared of under 3%. London is a notable exception, with extraordinarily

high overcrowding incidence: over 17% across all households, compared to a national average of 11%. In the second graph, I restrict the sample to those households in social housing. This time, there is a strong *positive* relationship: the incidence in the highest-wage cities is about 20%, but only 15% for the lowest. Therefore, low-productivity areas tend to have more - and better quality - social housing, another draw for the unemployed to remain.

Of course, all these results are merely correlations, and more work needs to be done to understand where they come from - especially in terms of general equilibrium.

## 4.7 Migration patterns to and from London

As discussed in Section 4.5 above, the combination of rising prices and unemployment in London is suggestive of a prolonged shock to labour *supply*. One possible candidate is immigration. In Figure 4.7, I plot the evolution (over 1980-2008) of net inter-regional migration rates (in blue) and net international migration rates (in red) by region, based on ONS data (see Section 4.2 for further details). I have borrowed this method of presenting these results from The Economist (2003), but extended the analysis to other regions in the country apart from London. All rates are expressed as a percentage of the particular region's population.

London is clearly exceptional: net foreign migration is much higher than elsewhere. It was zero in the early 1980s, but has been increasing steadily since, reaching about 1% in the early 2000s; it has shrunk somewhat since the latest recession. In contrast, London's net *inter-regional* migration rate has been consistently negative, and almost a mirror image of the international rates, peaking at below -1% in the early 2000s.

So, while foreign migrants have been entering London en masse, there has been a large exodus of Londoners to other parts of the country. This is not a new observation (see e.g. The Economist, 2003; Gordon et al., 2007), but my contribution is to link it with employment patterns. There is evidence that immigration has put downward pressure on the wages of low skilled Londoners (Dustmann et al., 2013), and it may have contributed to rising housing costs - amplified by significant constraints on local housing expansion (see Hilber and Vermeulen, 2010). Certainly, foreign investment in London property from Asia and the Middle East has also played a (probably more) important role (The Economist, 2012). The result of all this is falling real wages, which may have encouraged many Londoners to leave the city. It also possible that this has driven some Londoners to benefit dependency (and housing benefit in particular, which is linked to local rents) to support themselves in place of long-term work. This is consistent with the story from Chapter 2 of migrants undercutting native wages in the labour market. But of course, this is all merely speculation.

It is worth checking which demographic groups are responsible for the patterns we see in London. Ideally, it would be good to decompose the migrant groups for each year separately to observe their evolution. However, the LFS samples are too small to carry out a within-region analysis. Instead, I focus on the 5% sample available from the UK census of 2001. The census asks respondents for their current region and in which region they lived in 12 months previously. This allows me to estimate annual migration rates.

In Figure 4.8, I plot annual migration rates to and from London by age group. All rates

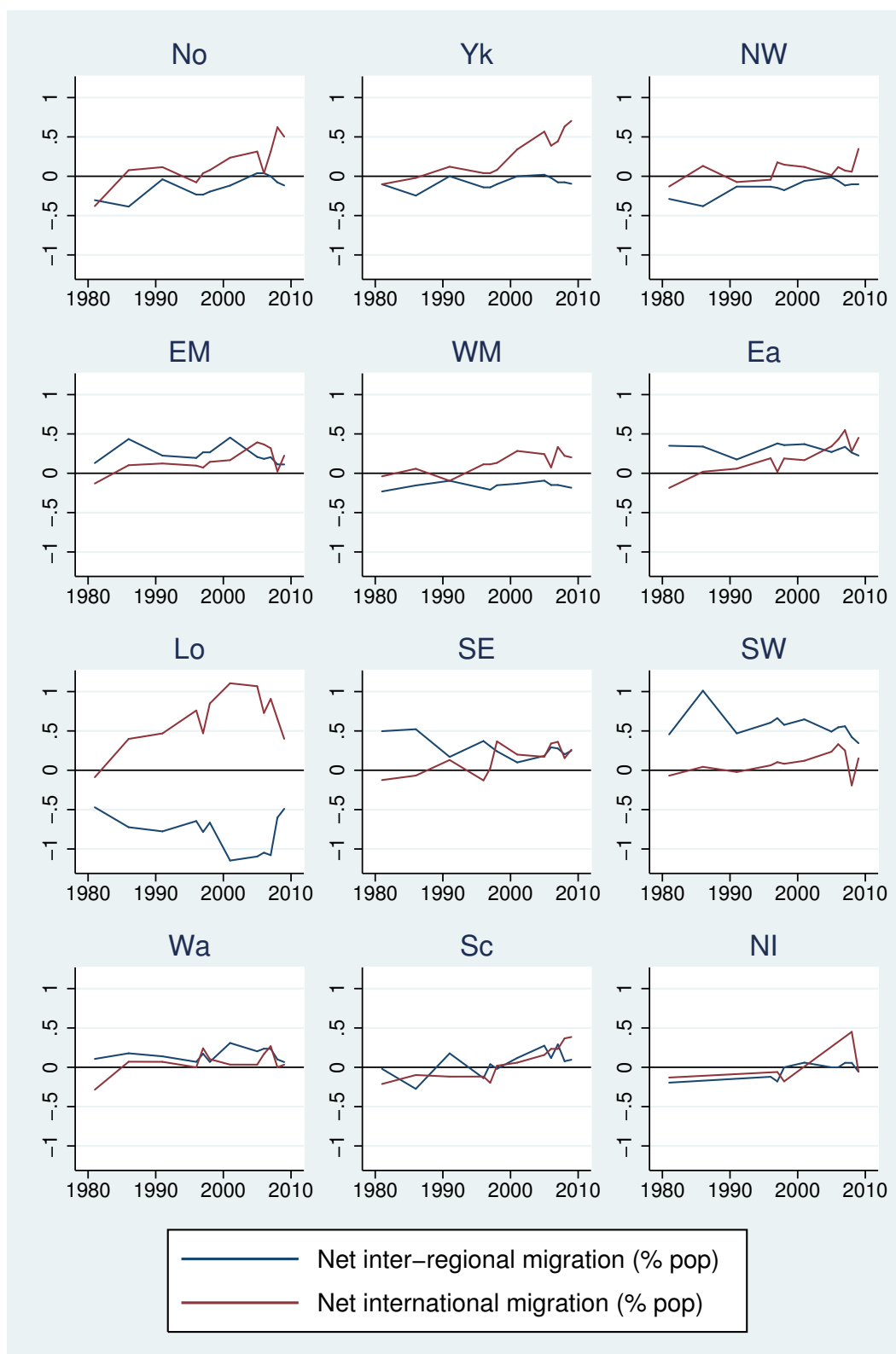


Figure 4.7: Inter-regional and international migration, by region

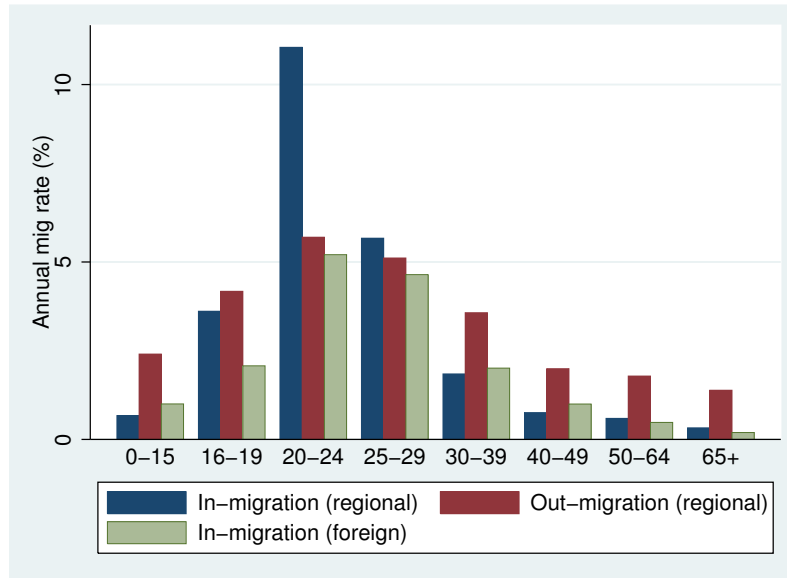


Figure 4.8: In- and out-migration rates for London, by age (2001)

are expressed as percentages of London's population in the particular age group. For each age category, the first bar represents the rate of in-migration to London from other parts of the UK (i.e. regional in-migration); the second bar is the rate of regional out-migration from London; and the third bar is the rate of in-migration to London from abroad. (Of course, it is not possible to observe the characteristics of foreign out-migrants in the census.)

Unsurprisingly, the bars are highest for individuals in their 20s: the young are known to be more mobile. But, looking at the difference between the blue and red bars, it is clear that the net regional out-migration from London is entirely due to the over-30s (and under-20s; but these are children who simply reflect the movements of their household heads). Older individuals are more likely to be homeowners, and many of the out-migrants will be capitalising their gains from rapid growth in London house prices. On average, net regional migration is -1.3% for the over-30s; notice that this statistic is driven by many individuals of working-age and not just retirees. In contrast, net regional migration to London is *positive* for 20-29s, and particularly large for 20-24s (5.4%). Immigrants make up a large proportion of total in-migration to the city (44%). And, the composition of foreign in-migrants also seems to be relatively dominated by the 20-29s. Incredibly, if we consider all forms of in-migration, 16% of London's population of 20-24s (the sum of the red and green bars) were living elsewhere 12 months previously.

In Figure 4.9, I repeat the same exercise but across education groups, for the sample of 16-64s. Skilled workers tend to be more mobile, and this is reflected by the higher bars on the right. Comparing the blue and red bars, we can see that the regional exodus from London is entirely due to lower skilled workers. For workers with NVQ level under 3 (i.e. less than A-level), net regional out-migration is -1.4%. In contrast, for those with NVQ levels 4/5 (i.e. with higher education), net regional migration is positive: 1.4% on average. Looking at the

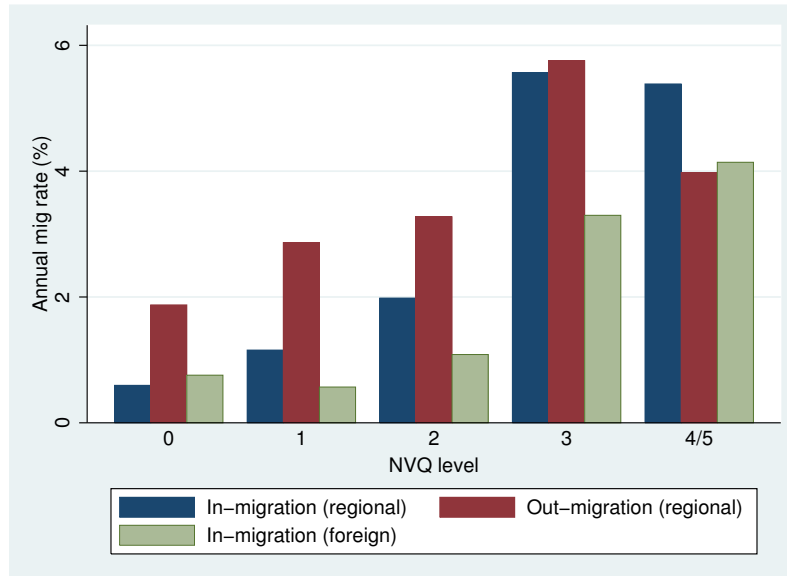


Figure 4.9: In- and out-migration rates for London of 16-64s, by education (2001)

distribution of the green bars, immigration from abroad also tends to be dominated by the high skilled. However, Dustmann et al. (2013) show that well-educated migrants commonly downgrade on arrival to the UK to lower skilled occupations.

Where in the UK have the London out-migrants gone? This is addressed by Figure 4.10. For each region, I plot the migration rate to London (in-migration) and the migration rate from London (out-migration), always as a percentage of the London population. It turns out that almost all of the net-migration out of London has gone towards its neighbouring regions, the East and South East. This opens up the possibility that many of these migrants have kept their jobs and are simply commuting longer distances from cheaper areas. Indeed, the evidence supports this theory. In Figure 4.11, I plot the distribution of commuting distance for two groups: (1) employed residents of the East and South East who lived in London 12 months previously and (2) all other employed residents of those regions. Many more of the ex-Londoners are commuting over 20km: over 40%, compared to under 20% for the rest of the local population.

## 4.8 Conclusion

In this chapter, I have documented key facts on local joblessness in the US and UK. In the US, local rates of unemployment and inactivity are unrelated to labour productivity, which is instead reflected in the local population. In the UK though, local employment shocks have persisted for many decades, and there is a strong correlation across cities between wages and joblessness (with the important exception of London, which has both high wages and high unemployment). I have speculated that the country's generous means-tested welfare system has made this persistence possible, and that the adjustment of local housing markets

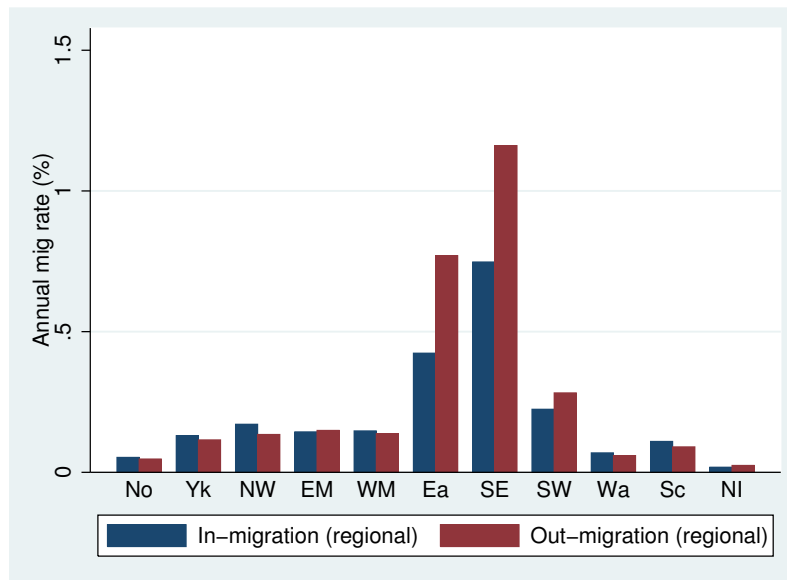


Figure 4.10: In- and out-migration rates for London, by region (2001)

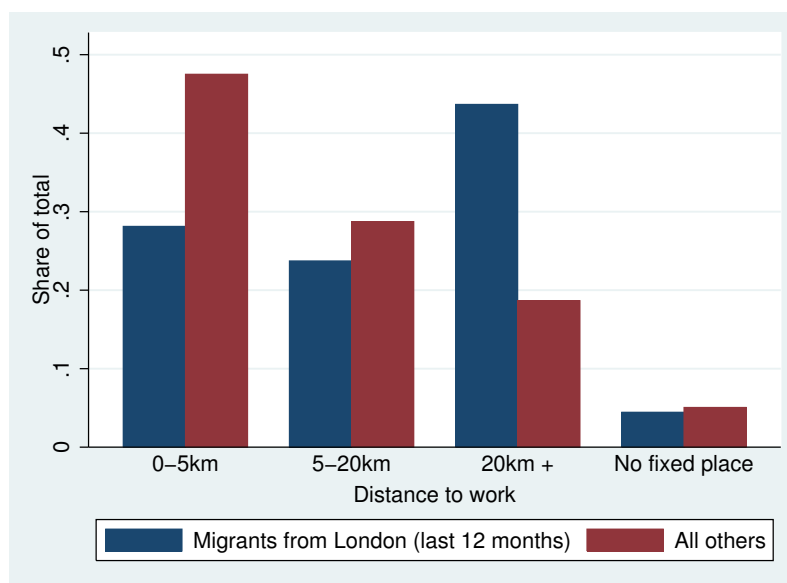


Figure 4.11: Commuting patterns of East and South East workers (2001)

has cemented it. While adverse demand shocks have brought about this situation in the old industrial towns, London appears to have experienced a prolonged supply-side shock. A possible candidate for such a shock is persistently high immigration (pushing down low skilled wages), together with large foreign investment in local housing. These factors may have encouraged Londoners to leave or resort to welfare, given that low skilled pay is often insufficient to support a reasonable life style.

This hypothesis is merely speculative, but if accurate, the clearest policy solution to the problems posed in this chapter would be to liberalise the local housing supply. Hilber and Vermeulen (2010) argue that much of the growth in house prices in the UK has been due to artificial planning restrictions. This is particularly hazardous in job-creating areas, as it impedes the adjustment of employment. However, this must be balanced with any preferences to preserve the British rural environment, and there are also many political obstacles to this kind of policy.

Another area of discussion is welfare policy, currently in a state of flux with the introduction of Universal Credit. Reducing the generosity of means-tested benefits to American levels would certainly help reduce persistence, but this may not be the optimal approach: the possible downside is a Detroit-style scenario. Interestingly, the government has recently discussed the idea of introducing regional variation in benefit payments (these are largely invariant geographically, with the exception of housing benefit). While this may help ameliorate poverty in many expensive areas, it could be detrimental to the employment situation in London if it makes the welfare alternative more attractive.

A more sensible approach to addressing the problems in London may be on the earnings side: regional pay in the public sector and more widespread application of “living wages” (or a nationally varying minimum wage) should help improve the returns to employment in the city. There may also be a role for strategic investment in social housing in terms of location: investment in job-rich parts of the South could help to encourage in-migration. Housing benefit may be counterproductive in this respect, for example, in allowing jobless families to survive in expensive London.

This work is currently purely descriptive, and the next step is to develop a rigorous model to test the theory. More immediate empirical concerns would be to estimate local real wages at different points in time since the 1980s, in the style of Gibbons et al. (2011), to see how they have evolved. It would also be useful to calculate the returns to employment in different cities, based on a rigorous analysis of local prices and the tax and benefit system - for both the UK and US.

# Chapter 5

## Do Households Use Homeownership To Insure Themselves? Evidence Across US Cities<sup>1</sup>

### 5.1 Introduction

Are households more likely to own their home when “housing risk” is higher? There is a large literature on how homeowners use home equity to smooth the transmission of earnings shocks into consumption<sup>2</sup>. In this chapter, we explore how the decision to become a homeowner is influenced by exposure to housing market risk and motives for insurance.

Some have argued that households may bring forward their home purchase as a hedge against future house price fluctuations (e.g. Sinai and Souleles (2005); Banks et al. (2010)). But, an earlier purchase would often necessitate a larger mortgage and increased risk to consumption. Using a life-cycle model, we argue that, in response to differences in housing risk, otherwise similar households are more likely to differ in their liquid savings than the timing of their ownership decision. Empirically, these savings differences manifest themselves in observed variation in loan-to-value ratios (LTV). In other words, in response to higher price risk, households do not bring forward ownership decisions (nor do households that never own choose to become homeowners) - but rather, conditional on owning, they may instead reduce their LTV.

Theoretically, when markets are incomplete there are several reasons why homeownership may be a peculiar and attractive form of insurance against certain risks in the housing market<sup>3</sup>. In Ben-Shahar (1998), Nordvik (2001), Sinai and Souleles (2005) and Ortalo-Magne

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<sup>1</sup>This chapter is based on joint work with Jonathan Halket.

<sup>2</sup>Hurst and Stafford (2004) and Hryshko et al. (2010) look at how households use mortgage refinancing decisions and home equity, respectively, to smooth unemployment shocks and earnings shocks. Leth-Petersen (2010) finds that household expenditure increases moderately after credit constraints are relaxed. Paciorek and Sinai (2012) finds that the cross-sectional variance of housing consumption is lower for homeowners that have moved between cities whose house prices are strongly correlated.

<sup>3</sup>Formal, direct means to insure against changes in house prices are limited (Caplin et al., 1997), and the



and Prat (2010), households may use homeownership to insure themselves against the risk of changes to the local rental price (or user-cost) of housing. However, Ortalo-Magne and Rady (2002) suggest that if a household’s expected future earnings are more strongly correlated with local house prices, then it already has partial insurance through their labor earnings.<sup>4</sup> Ortalo-Magne and Rady (2006) and Banks et al. (2010) propose and find supporting evidence for a housing-ladder theory in which households that plan on eventually owning a large house (in part because larger houses may not be available on the rental market) are more likely to own a smaller home (rather than rent) first if they live in a risky area. In this case, homeownership partially insures young households against increases in the price of a good in their future consumption bundle (the larger house).

If financial constraints prevent some households from insuring themselves through owning, there may be important welfare improvements from policies designed to make ownership “accessible”. However, measuring the size or even the overall sign of the insurance motive on homeownership is challenging, in part because it is difficult to isolate differences in households’ exposure to housing risks that are independent from other factors that affect their homeownership decisions.

This chapter proceeds in two steps. Firstly, we present some empirical facts based on cross-city<sup>5</sup> variation. As documented by Banks et al. (2010), households in high-risk cities are less likely to become homeowners. We also find that they are more likely to make a large down payment (in percentage-of-house-value terms) when they do buy. These relationships hold when controlling for household characteristics. However, causal inference is confounded by house price levels, which are systematically correlated with housing volatility in an intuitive way: in cities where the land value is larger relative to the local cost of structures, house prices are higher and more volatile. When we look at the variation in homeownership rates and LTV by land share (the ratio of local land values to total housing costs), we see the same strong negative relationship. This is true even after instrumenting for possible endogeneity, using a measure of physical local land scarcity constructed by Saiz (2010). So, secondly, we use a quantitative life-cycle model with homeownership to disentangle the effects of higher risk from higher price levels on the life-cycle timing of homeownership and mortgage decisions.

We focus on the cross-sectional dimension in our empirical work, rather than the time-series, for several reasons. For one, in the data, the amount of heterogeneity both in household and price behavior is much larger across cities than within cities over time. For another, the land scarcity instrument offers a way to measure the effect of higher price levels and risk (jointly) on household behavior in the cross-section. However we cannot use it to control for endogeneity within a city over time. In this sense, our work is complementary to much of

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correlation between house prices and other financial assets is small (Flavin and Yamashita, 2002).

<sup>4</sup>Davidoff (2006) finds that households purchase less housing when they work in an industry whose workers’ income are relatively more correlated with local house prices. However, he finds very small effects of the same on the probability of homeownership.

<sup>5</sup>Throughout we refer to Metropolitan Statistical Areas as “cities” and “LTV” always refers to the loan-to-value at origination (that is, at the time of purchase). We will sometimes refer to the time-series standard deviation of the annual changes to log house prices within a city as its “volatility”.

the previous literature (e.g. Sinai and Souleles (2005)), which exploits within-city variation in volatility (controlling for prices) to measure the insurance motive.

In order to measure the effect of higher volatility on homeownership, we disentangle its impact from that of higher prices by building a life-cycle model of homeownership choice. We account separately for innovations to house prices that are correlated with city wages and those that are not. The model has a flexible housing ladder where medium-sized housing can either be rented or owned, which enables the model to match the relative consumption of owner-occupied to rental housing in the average city according to land scarcity. Importantly, households have another means of imperfectly insuring themselves in addition to homeownership: a risk-free bond.

In our setup, households have several potential reasons why they might use homeownership to insure against housing risk. They may use homeownership to insure themselves against the risk of changes to the rental price of housing, though their labor earnings will provide some partial insurance. Also, the housing ladder assumption forces households that wish to live in large houses to own them so that the model nests the theories of Banks et al. (2010) and Ortalo-Magne and Rady (2006). Otherwise, the basic elements of our life-cycle model of homeownership are similar to those in Cocco (2005), Li and Yao (2007) and others.

Key model parameters are chosen to match moments from the average city based on land scarcity. Cities in the model differ *ex ante* only in their land scarcity and, through land scarcity, their stochastic processes for house prices and wages. The model endogenously captures most of the variation by land scarcity in household behavior. Given that, we then perform counterfactual analyses where we vary one element at a time (e.g. vary the level of prices, keeping volatility constant). We find that most of the observed variation in homeownership across cities comes from the observed variation in house price levels and not the variation in risk. Instead, higher risk leads to slightly lower LTVs and homeownership rates in the model.

The weak effect of risk on homeownership comes despite the large dispersion across cities in the local volatility of house prices, which is as large as the dispersion in price levels. Furthermore, absent other factors, more risk would lead to more homeownership in the model (and as in Ortalo-Magne and Prat (2010)); homeownership is the only asset available for purchase in the model that has returns correlated with any shock. However in our model, homeownership also has many extra costs that potentially increase with more price volatility (such as the transactions costs for buying a house). Moreover, households also have an alternative to using homeownership for insurance: they can accumulate precautionary, non-housing savings instead. We find that, given the extra costs to homeownership, young households with rising income profiles would rather save a little in liquid precautionary savings than save a lot to afford a down payment. These extra savings help explain the lower LTV ratios in the high-risk cities. The precautionary savings motive is not sufficient in our model to generate the same magnitude of LTV dispersion as in the data so it is likely that there are other explanations beyond those discussed here that are also important in explaining the dispersion in mortgage behavior. The qualitative concurrence of volatility and LTV in the model and data should then be treated as corroborating our other, more

definitive evidence that higher housing risk does not lead to more to more homeownership.

In the model, dispersion in price levels has a much larger effect than risk on homeownership choices due to the housing ladder. With a housing ladder, a household in our model must own (rent) if it wants to live in a particularly large (small) house. Higher prices in a city decrease housing consumption and, therefore, reduce homeownership rates.

Patterns in the data corroborate our conclusions that differences in price levels cause differences in homeownership rates through housing ladder effects, while differences in LTV are independent of housing ladder effects and are instead due to risk. In the data, once we condition on whether a household lives in an apartment or a house (a proxy for the housing ladder in the model), the negative correlation between homeownership rates and price levels disappears. However, the negative relationship between LTV and volatility does not disappear after conditioning.

In the last section of the chapter, we discuss why regression-based inferences of risk's effect on homeownership may be biased. Homeownership decisions in economies with transaction costs are durable decisions. Unsurprisingly for an (S,s)-type model, not only contemporaneous prices but also the past history of prices help determine whether a household currently owns or not. Therefore, homeownership rates within the city economy are also a function of the history of prices. In many studies, expected housing risk is measured using the volatility of area house prices around the time that the homeownership rate is measured<sup>6</sup>. Thus, the volatility variable picks up the history dependence of homeownership on price levels, leading to potential bias.

## Related literature

A key contribution of this chapter is that we document systematic cross-city variation in homeownership and LTV using both micro and aggregate (city-level) data. Banks et al. (2010) use both variation within and across U.S. states and U.S.-U.K. comparisons on homeownership. Chiuri and Jappelli (2003) look across developed countries for the effect of financial market imperfections on homeownership. Albouy (2009a) and Albouy (2009b) look at the effects of cross-city variation in taxes and amenities. Han (2010) looks at the effects of housing risks on housing demand and homeowners' propensity to move, using cross-city and time variation. City-level data is appealing since it is more plausible to assume, as we do, that financial market conditions are similar across the areas, in contrast to cross-country comparisons. But there is still enough plausibly exogenous, observable variation in price levels and risk across cities to find systematic differences in household choices.

Han (2008) builds a model where homeowners may choose to accumulate more housing so as to hedge against housing risks. Under the assumption of separable utility and no income risk, she provides conditions for when the hedging motive outweighs the household's normal disinclination to hold riskier assets (as in Rosen et al. (1984)). Han (2013) finds evidence

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<sup>6</sup>For example, Sinai and Souleles (2005); Banks et al. (2010). Han (2010, 2013) look at the variance of house price forecasts instead of house prices where prices are forecasted using an AR(1) process and the variance of innovations is forecasted using a GARCH(1,1) process.

that the hedging motive may be priced into housing risk premia in markets where housing supply is constrained. Our work expands on this contribution by adding the option of renting and looking at homeownership and borrowing behavior jointly.

There are a few studies that examine the opposite causal direction - the effect of homeownership and borrowing decisions on prices. Stein (1995) proposes a model where price changes have asymmetric effects on sales due to down payment constraints. Lamont and Stein (1999) find that cities with high LTVs have higher rather than lower elasticities of house prices with respect to changes in income, but the instrument they use turns out to be weak. Genesove and Mayer (1997) find that within a specific market (the Boston condominium market), sellers with higher LTVs have higher expected time on the market and receive higher prices.

We do not offer a general equilibrium model of housing; neither do we deal with issues of regional mobility<sup>7</sup> or time variation in the stochastic process for prices. By using the time-invariant differences in land scarcity across cities to calibrate the different price processes, we largely sidestep issues of endogeneity that may normally arise from examining only one side of a market. Providing structural explanations for the relationship between land values and house prices and for the existence of a housing ladder are interesting explorations that we hope the facts presented here will encourage.

The rest of this chapter is as follows: Section 5.2 shows the striking variation in homeownership, LTV, house prices and housing risk across U.S. cities, Section 5.3 presents the model, and Section 5.4 discusses its parametrization. Section 5.5 presents our results. Section 5.6 discusses bias in regressions and concludes. We include appendices with further details on our data work and model parameterization.

## 5.2 Homeownership and loan-to-value ratios in the data

In this section, we present some basic facts from cross-city data. First, we show that local homeownership rates are decreasing in price volatility (as Banks et al. (2010) document). But, we cannot draw causal inference from this result, because local price volatilities are themselves closely correlated with local price levels. And indeed, homeownership is also strongly negatively correlated with price levels across cities.

We then show that cities with high and volatile prices are also characterized by low LTV ratios. So, households in these cities are less likely to own. And when they do choose to own, their purchases are less leveraged. But, again, it is not clear solely from the data whether these outcomes are insurance-type responses to local price volatility, local price levels or something else entirely.

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<sup>7</sup>See Halket and Vasudev (2013) for a model with both, but without the changes in housing supply that we would need here to close our model. Paciorek (2012) has a model of housing supply where the elasticities differ according to factors like land scarcity. Lustig and Van Nieuwerburgh (2010) looks at inter- and intra-regional risk sharing and home values but not homeownership.

Local price levels and volatilities are closely related for a simple intuitive reason. They share the same statistical source: variation in land share, i.e. the share of the price of the city’s typical house that is attributable to the value of land (as opposed to the cost of the structure). As a result, it is not possible to empirically disentangle the impact of price volatilities from that of price levels without the use of behavioral models and counterfactuals.

## Data

This chapter is based on a number of data sources, with our analysis restricted to the cross-section of 2000 for simplicity<sup>8</sup>. Ownership rates and local mean price levels (based on reported values of owned dwellings) are constructed from the Integrated Public Use Micro-data Series (IPUMS) 5 percent extract of the US 2000 census, organized by Ruggles et al. (2010b). We use two different measures of LTV ratios, taken from the American Housing Survey (AHS) and the Monthly Interest Rate Survey (MIRS)<sup>9</sup>; the latter is maintained by the Federal Housing Finance Agency (FHFA). Quarterly metropolitan house price indices are also taken from the FHFA<sup>10</sup> to estimate local price volatilities. And, our metropolitan-level average earnings series come from the Bureau of Economic Analysis’ (BEA) regional program<sup>11</sup>. Finally, we use data on local land scarcity from Saiz (2010) and land share from Davis and Palumbo (2008); we discuss these further below. Where survey data are used, we restrict our sample to households with heads aged 21-75<sup>12</sup>, living in houses or apartments. Throughout this chapter, we weight all city-level regressions and scatter plots; our weights correspond to the local sample of households with heads aged 21-75, living in houses or flats (as estimated from the census extract).

We identify cities with the set of (Primary<sup>13</sup>) Metropolitan Statistical Areas (MSAs), of which there are 297 in the census data in 2000. However, we restrict our sample to the 221 MSAs for which FHFA price data, BEA wage data and the land scarcity instrument are available. Of these, 42 cities are available in the AHS (for the estimation of local LTV ratios) and in the Davis-Palumbo (2008) data on land shares (itself based on the AHS). And, just 25 are available in the metropolitan MIRS LTV data - though these tend to be the most prominent cities. Further details on the city sample can be found in Appendix A.

Our measure of local house price volatility is the standard deviation of log annual changes in the FHFA local price index (measured in the first quarter of each year) over the previous five years (1995-2000).<sup>14</sup> This approach is based on Banks et al. (2010). We estimate wage volatility in the same way using annual BEA data.

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<sup>8</sup>In Appendix C, we show that the general patterns also hold for 1990, with the exception of the systematic patterns of LTV discussed below which have become considerably stronger since 1990.

<sup>9</sup><http://www.fhfa.gov/Default.aspx?Page=250>

<sup>10</sup>All-transactions index; <http://www.fhfa.gov/DataTools/Downloads/Pages/House-Price-Index-Datasets.aspx>. The Case-Shiller indices distributed by Standard & Poor’s (which is the other popular publicly available data set) cover a much smaller sample of cities.

<sup>11</sup>[http://www.bea.gov/iTable/index\\_regional.cfm](http://www.bea.gov/iTable/index_regional.cfm)

<sup>12</sup>In the case of the AHS, age is calculated at purchase year.

<sup>13</sup>We do not aggregate these into “consolidated” areas.

<sup>14</sup>Appendix C contains robustness results for alternative volatility window lengths.

We present much of the evidence on cross-city correlations between ownership rates/LTV and price levels/volatilities graphically. But, there are of course concerns that any observed effects will simply be driven by differences in local household composition. Therefore, in all cross-city analysis (including graphs), we use local ownership rates and LTV that condition on local household characteristics and household income in particular. See Section 5.6 for more details.

There are concerns of large measurement error in the LTV data estimated from the AHS (Lam and Kaul, 2003). And so, we also present our analysis using MIRS data. The MIRS reports (among other statistics) mean LTV ratios for conventional (i.e. excluding federally guaranteed Federal Housing Authority (FHA) and Veterans Administration (VA) loans; see Appendix A for description of loan types) single-family loans in 25 cities, based on a monthly survey of mortgage lenders. However, the AHS does have a number of advantages for our purposes: it covers more cities, it covers non-conventional loans also, and (being a household survey) it allows us to control for household characteristics.

Our data on land shares are taken from Davis and Palumbo (2008)<sup>15</sup>. They construct a data set containing, by city and quarter, the average local house price, as well as the share of the local price that is attributable to land value and structure cost, respectively, so that:

$$\begin{aligned} housevalue_{j,t} &= landvalue_{j,t} + structurevalue_{j,t} \\ l_{j,t} &= \frac{landvalue_{j,t}}{housevalue_{j,t}} \end{aligned}$$

where  $l_{j,t}$  is then the land share for city  $j$  at time  $t$ . Their land value estimates are the residual part of house values within a city that is not explained by structure costs. Since it is partially based on the AHS, these data are only available for 42 MSAs in our sample.

As a supply-side instrument for the share of the price attributable to land, we adopt Saiz's (2010) measure of local land scarcity, based on physical constraints on housing supply. For each city, this is the share of a circle around the city center, of 50km radius, that is either steeply inclined land (at an incline of over 15 percent) or water.<sup>16</sup> Saiz estimates this variable with satellite data.

## Homeownership and LTV

As the first panel of Figure 5.1 shows, the local homeownership rate (conditional on household income and other characteristics) is negatively correlated across cities with house price volatility. The predicted (OLS) effect shows ownership rates ranging from about 0.7 (for the least volatile cities) to 0.4 (for the most), with an R squared of 41 percent. But, as the second panel shows, it is also strongly negatively correlated with price levels (see the first two panels of Figure 5.1): here, the correlation is 60 percent. It should be noted that

<sup>15</sup>Their data are available at <http://www.lincolnst.edu/subcenters/land-values>.

<sup>16</sup>Mian and Sufi (2011) and Chaney et al. (2012) similarly use this data to instrument for elasticities of supply, while Paciorek (2012) builds a model of housing supply that directly connects Saiz's measure of land scarcity to the theoretical elasticity.

New York appears to be an important outlier in these homeownership figures; however, we argue in Section 5.5 that this relates to the local abundance of apartments and the results presented here are anyway robust to excluding it.

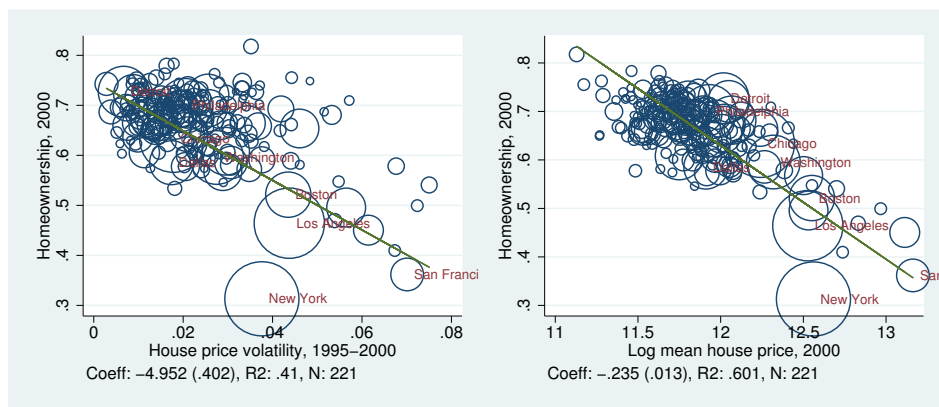


Figure 5.1: Homeownership, price risk and price levels

Unsurprisingly, volatilities and levels are themselves closely related, with a correlation of 38 percent (see Figure 5.2). Consequently, it is difficult to disentangle their respective effects. To see this graphically, we isolate the portion of variation in volatilities that is uncorrelated with price levels (i.e. the residuals from an OLS regression of volatilities on levels). In the first panel of Figure 5.3, we plot homeownership against these volatility residuals: the effect is much weaker than before, with less than half the coefficient and an R squared of under 5 percent. In the second panel, we plot homeownership against price level residuals (from a regression on volatility). The relationship is stronger than the one from the volatility residuals<sup>17</sup>, though still much weaker than in Figure 5.1: the correlation is 23 percent.

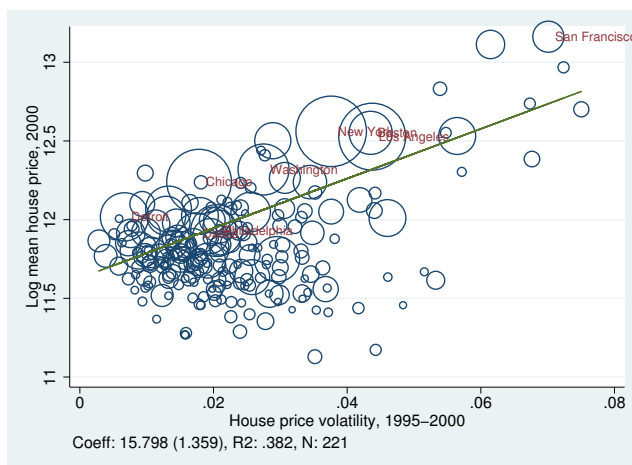


Figure 5.2: Price levels and risk

<sup>17</sup>This is perhaps unsurprising given that volatility is measured with more error than levels.

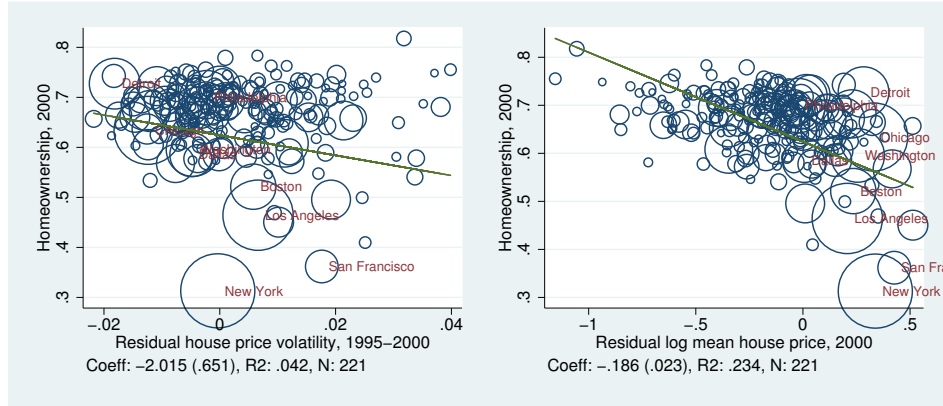


Figure 5.3: Homeownership and residual price levels and volatilities

As with homeownership, average city loan-to-value ratio at origination (LTV), as measured by the AHS (controlling for household characteristics including income), is strongly negatively correlated with both local price volatility and level (Figure 5.4). In the first panel, moving from the lowest to highest price volatilities in the MSA sample, following the OLS-predicted line, LTV falls from 0.85 to 0.79, with an R squared of 19 percent. But again, unsurprisingly, the second panel shows a strong negative correlation between LTV and price levels of 46 percent.

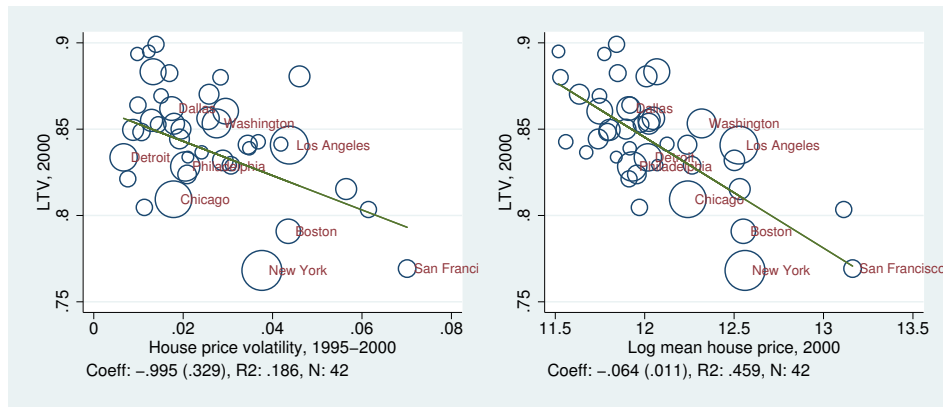


Figure 5.4: LTV (AHS), price risk and price levels

These relationships also exist for the MIRS data, as Figure 5.5 shows. The correlation is much tighter than for the AHS data: the AHS is likely to be subject to substantial sampling and reporting error. But, the magnitudes of the effects on LTV are very similar for the two datasets: for example, for volatility, the MIRS effect is -1.2 compared to -1.0 for the AHS. Notice that the mean LTV is lower for the MIRS (0.77) than for the AHS (0.84) estimates. This is in part because the MIRS sample is restricted to conventional loans only: the AHS mean for conventional loans is 0.80.



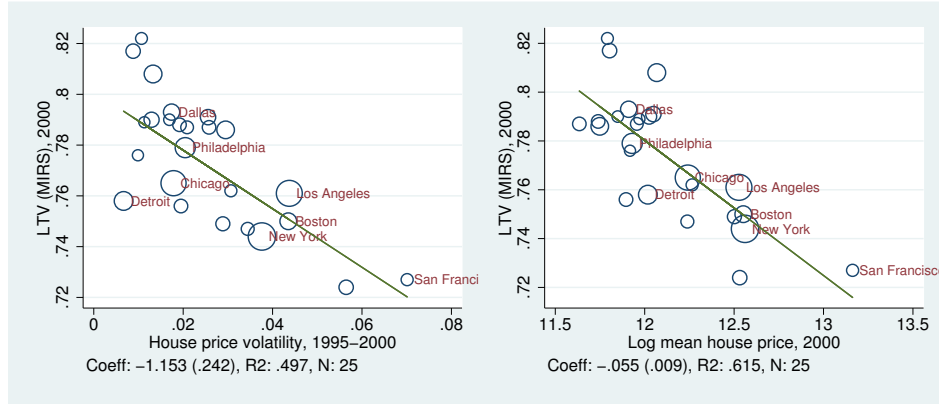


Figure 5.5: LTV (MIRS), price risk and price levels

It might be argued that these LTV patterns have a supply-side explanation, due to the intricacies of American mortgage institutions. But, in Appendix A, we show that geographically non-varying conforming loan limits do not drive the observed cross-city variation in LTV, and nor do differences in local mortgage interest rates. It is unlikely then that any potential geographic differences in default propensities are causing the observed variation in LTV via differences in default risk-premia. So in the model, we abstract from default.

Also, there may be concern that the variation in LTV is merely arising from cross-city differences in shares of mortgage-holders. Indeed, almost a quarter of homeowners (in the 5 percent census extract of 2000) do not hold mortgages. However, it turns out that the local mortgage share (among homeowners) is uncorrelated with the ownership rate itself, so it is not likely to be driving our results. Lastly, we also show in Appendix A that the LTV patterns are equally strong for first-time buyers as for repeat buyers, so our results are unlikely to arise from existing homeowners trading up after periods of high price growth.

To summarize, expensive and price-volatile cities tend to be characterized by low ownership rates, but also low LTV ratios. Households in these cities are less likely to own, and when they do buy a house, they take a bigger equity stake. But, we cannot make causal statements based on this evidence, given the close association between local price volatility and levels.

## Association between house price volatility and levels

To understand the close link between volatilities and levels, it is necessary to view house prices as the sum of two components: land values and structure costs. Price volatilities and levels are correlated across cities because they share the same statistical source: variation in local land shares, as demonstrated in Figure 5.6.<sup>18</sup> The intuition is simple: compared to structure costs, the price of land tends to vary much more both across cities and within cities

<sup>18</sup>To ensure comparability, all data (including house prices) for this figure are taken from Davis and Palumbo (2008) - for 42 MSAs.

over time; as a result, due to a simple composition effect, cities with large land shares tend to have higher and more volatile prices. We elaborate on this in Appendix A.

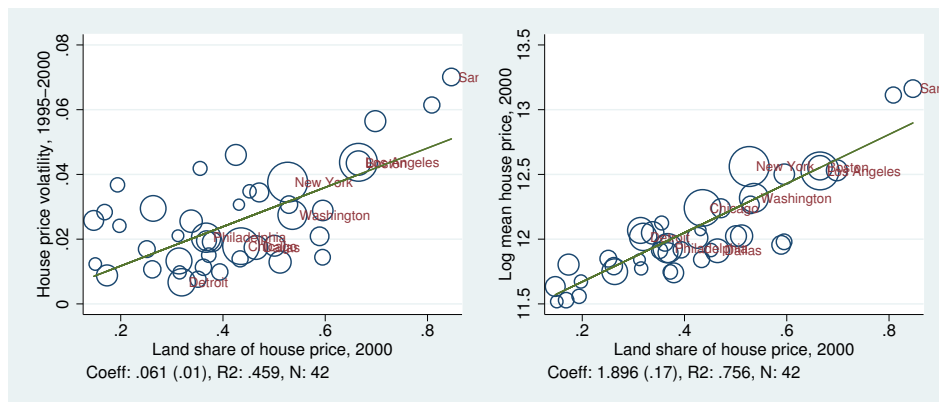


Figure 5.6: Price risk and levels and land share

Given the strong empirical connection between price volatility and levels, we have to disentangle their effects on household choices with a model. One approach would be to simulate ownership and LTV decisions in cities with different land shares. These cities are characterized by different local price volatilities and levels (this is what matters for the simulation), and these can be estimated from the data (by reference to their empirical relationship with land share).

The problem is that the correlation between land share and ownership or LTV cannot be considered causal *a priori*. Omitted variation in local productivity or housing demand should not be a concern, because we are controlling for household income in our estimates of ownership and LTV. But, we are worried about reverse causation from ownership/LTV to price levels/volatilities and land share.

There are two ways to address this issue. The first is to simulate a general equilibrium model, where land share, price levels and volatilities are all determined endogenously. However, the determination of these housing market outcomes is not the focus of this chapter, and the equilibrium conditions would significantly complicate computation.

The second approach, which we choose, is to find a suitable instrument for land share that will only affect ownership and LTV indirectly, i.e. via local price levels and volatilities. We opt for a measure of local land scarcity, described in the data section above. It is based on local geographical features, namely inclined land and water. Conditional on household income, this instrument is unlikely to affect tenure and LTV in a significant way *directly*; the effect should only come through local housing conditions (captured by price levels and volatility). The first stage is sufficiently powerful. Figure 5.7 shows a strong relationship between land scarcity and land share: a 1 percentage point increase in land scarcity is associated with a 0.5 percentage point increase in land share. Unsurprisingly, Figure 5.8 shows there is a strong positive relationship between land scarcity and price volatilities/levels as well, with correlations of 28 and 30 percent respectively. Finally, Figures 5.9 and 5.10 show the relationships between homeownership and LTV and land share/scarcity.

As with land *share*, we confirm in Appendix C that the entire effect of land *scarcity* on price levels comes through the land value component (and not structure costs). And, the effect on price volatility is entirely a composition effect: land scarce cities have larger land shares, and local land values are more volatile than structure costs.

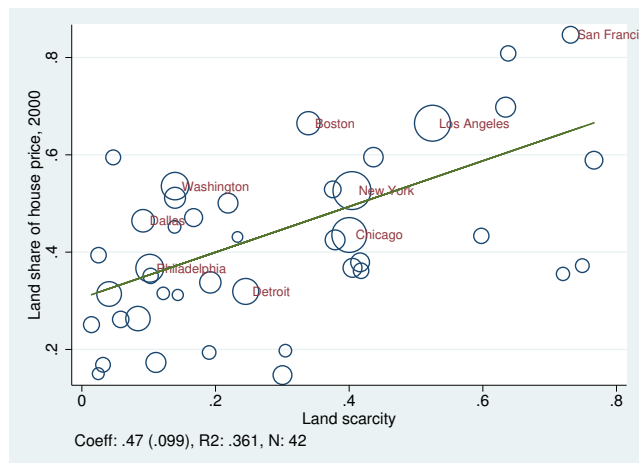


Figure 5.7: Land share and land scarcity

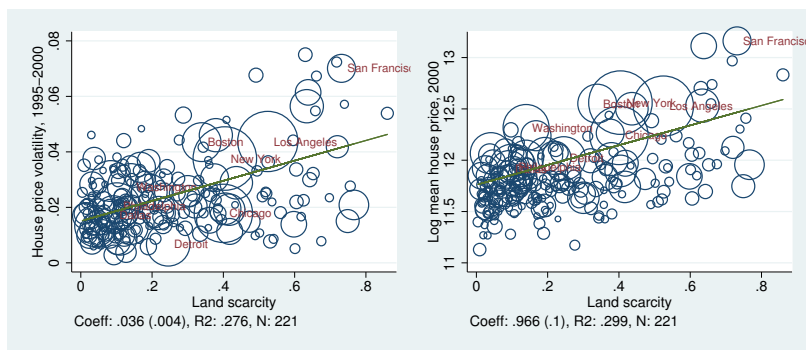


Figure 5.8: Price risk and levels and land scarcity

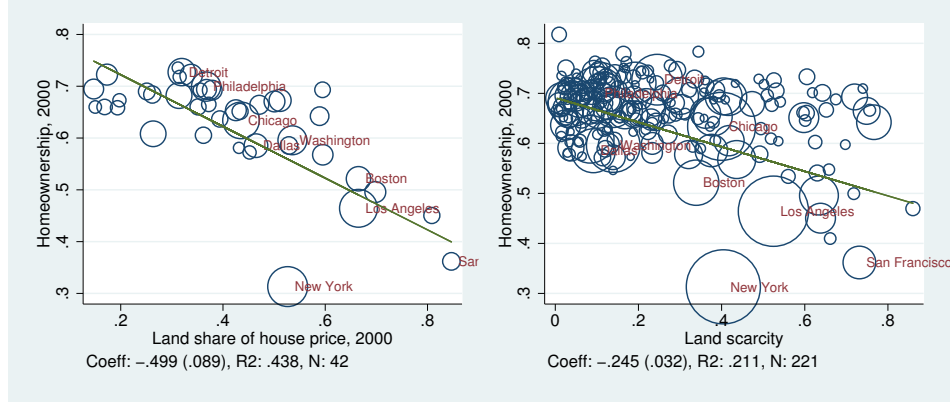


Figure 5.9: Homeownership, land share and land scarcity

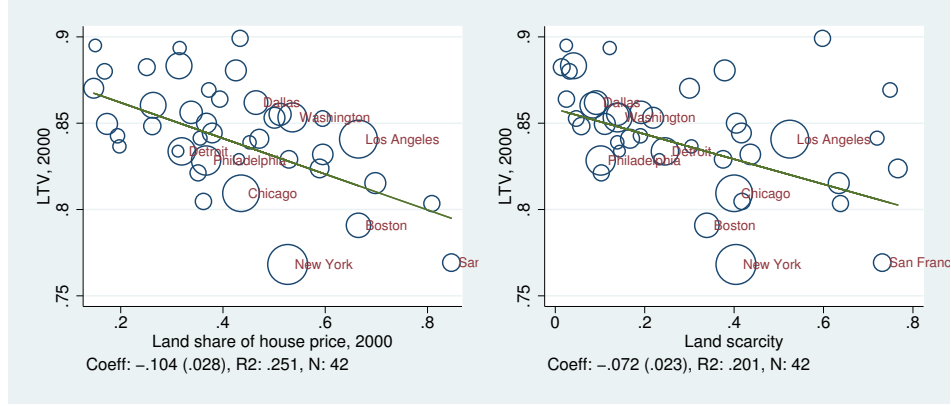


Figure 5.10: LTV, land share and land scarcity

### 5.3 Household choice model

In this section, we build a life-cycle model of households that work and consume in a particular city for their entire lives. Several of the assumptions we make deserve extra attention.

We severely limit households' access to insurance in a way that should bias the model *in favor* of using homeownership as insurance: we do not allow for inter-city migration, so households cannot use moving away from the city as a source of insurance against house price changes<sup>19</sup>; and the only asset besides a house is a risk-free bond.

Though the model is “partial-equilibrium”, rental prices are tied to sale prices through an implied equilibrium relationship that leads to counterfactually high rental volatility. In the model, rents will be as volatile as house prices, while in the data they are clearly lower<sup>20</sup>.

<sup>19</sup>See Sinai and Souleles (2009) for a model where owning can hedge moving risk. There the risk is that a household moves to a market whose house prices are highly correlated with the household's previous market. In our model, all moves will be within-city moves and so this risk is maximized.

<sup>20</sup>For instance, see Campbell et al. (2009) and Verbrugge (2008).

Excess volatility in rents as compared to prices will again bias the model in favor of homeownership as insurance. Also, all house price changes are common to all houses within a city; we abstract away from house-level idiosyncratic changes in prices and rents. This too favors the homeownership as insurance hypothesis as idiosyncratic volatility would fall more heavily on homeowners in the model (a renter could easily move to an alternative house if she gets an idiosyncratic increase in rent).

Time is discrete, and each period in the economy corresponds to one year in the data. Households are born at age  $a = 21$  and live at most to age  $a = 75$ . A household is indexed by  $i$  and lives in a city, indexed by  $j$ , for its entire life. The city has a time-invariant land scarcity  $\lambda_j$ .

## Preferences

Households have recursive preferences of the Kreps and Porteus (1978) type<sup>21</sup>. The household gets instantaneous utility from a non-durable consumption good  $c$  and a durable housing good  $h$  according to:

$$u(c_t, h_t, a_t) = (c_t^{1-\sigma} h_t^\sigma) / F(a_t)$$

The path for the family size adjustment factor,  $F : \{21, 22, \dots, 75\} \rightarrow \mathbb{R}_{++}$ , is exogenous, constant across households of the same age and known to the household at birth.<sup>22</sup> The household's utility at time  $t$ ,  $V_t$ , is then given by the composite of its instantaneous utility and its future expected utility:

$$V_t = [(1 - \beta)u(c_t, h_t, a_t)^{1-\phi} + \beta(\mathcal{R}_t V_{t+1})^{1-\phi}]^{\frac{1}{1-\phi}},$$

where future expected utility is given by  $\mathcal{R}_t V_{t+1} = (\mathbb{E}_t[V_{t+1}^{1-\gamma}])^{\frac{1}{1-\gamma}}$ .  $\gamma$  measures risk aversion while  $\phi$  is the inverse of the intertemporal elasticity of substitution. Additive utility is a special case where  $\phi = \gamma$ .

Households get utility at death from bequeathing wealth,  $V_{t+1}(\cdot, a_t = 75) = (b_{t+1} + p_{t+1}^j h_t)^{1-\sigma}$ .

## Labor earnings

Households receive labor earnings,  $Y_t^{ij}$  until an exogenously set retirement age  $R$ , after which they receive a pension.  $Y_t^{ij}$  contains two parts: idiosyncratic components and a city-specific

<sup>21</sup>These preferences nest time-separable preferences but allow for the separate consideration of intertemporal smoothing (savings) and smoothing across states within a given period (risk-aversion).

<sup>22</sup>Attanasio et al. (1999); Gourinchas and Parker (2002); Cagetti (2003); Li and Yao (2007) each let family size affect a household's discount factor. In Gourinchas and Parker (2002); Li and Yao (2007), the life cycle profile for family size is deterministic and homogeneous across households of the same age. Attanasio et al. (1999); Cagetti (2003) let the profiles vary by education. Browning and Lusardi (1996) have a stochastic process for family size (see their paper for more references). Gervais (2002); Campbell and Cocco (2003); Li and Yao (2007); Diaz and Luengo-Prado (2008) all use Cobb-Douglas preferences over non-durable consumption and housing that are consistent with evidence from Davis and Ortalo-Magne (2011) that housing expenditure shares are approximately constant across cities.

component. The city-specific component  $W_t^j$ , which we call wages, follows a geometric random walk. The idiosyncratic components are,  $L_t^i$ , a geometric random walk with deterministic age-dependent drift, and a transitory shock,  $\rho_t^i$ , as in, for example, Storesletten et al. (2004a):

$$\begin{aligned} Y_t &= L_t^i W_t^j \varrho_t^i \\ L_t^i &= \exp f(a_t) L_{t-1}^i \psi_t^i \\ W_t^j &= W_{t-1}^j \nu_t^j \end{aligned}$$

where  $\ln \psi_t^i \sim \mathcal{N}(-0.5\sigma_\psi^2, \sigma_\psi^2)$ ,  $\ln \varrho_t^i \sim \mathcal{N}(-0.5\sigma_\varrho^2, \sigma_\varrho^2)$  and  $\ln \nu_t^j \sim \mathcal{N}(\mu_\nu - 0.5\sigma_\nu^2(\lambda_j), \sigma_\nu^2(\lambda_j))$ . The variance of innovations to wages,  $\nu_t^j$ , can differ across cities according to their land scarcity; however all cities have common drifts. After retirement, the household gets a proportion (adjusted for growth in the city) of its final salary,  $Y_t = \zeta L_R^i W_t^j$ . All households' income is taxed at a rate  $t_y$ .

## Housing market

At any time, homes may either be rented ( $\tau_t^i = 0$ ) or owned ( $\tau_t^i = 1$ ), but not both simultaneously. There is a housing ladder that forces households to choose rented housing from the set  $H^r$  and owner-occupied housing from the set  $H^o$ .

Housing can be bought at a unit price  $p_t^j$ , which contains two components - one correlated with labor earnings and one uncorrelated with labor earnings:

$$p_t^j = Q_t^j W_t^j$$

where  $Q_t^j = Q_{t-1}^j \epsilon_t^j$  and  $\ln \epsilon_t^j \sim \mathcal{N}(-0.5\sigma_\epsilon^2(\lambda_j), \sigma_\epsilon^2(\lambda_j))$ . The variance of innovations to the uncorrelated component,  $\epsilon_t^j$ , like those of the correlated component, differs across cities; city-specific drifts remain common<sup>23</sup>.

An owner pays proportions  $t_p$  and  $\delta_j$  of the value of the house each period toward property taxes and maintenance, respectively. The housing maintenance means houses do not depreciate and the maintenance required may vary across cities. A household may not “build on” to its house; to adjust the size of an owner-occupied house, it must sell its current one and buy a new house. Each time a household buys a house, it pays a fraction  $\theta_b$  of the value of the house as a transaction cost.

A renter pays only the spot rental price per unit of housing  $s_t^j$ , which we set so that a risk-neutral landlord would be indifferent between renting or selling the house, subject to paying income tax on its rental income:

$$s_t^j = \frac{t_p + \delta_j + \frac{r_b - \mu_\nu}{1 + r_b}}{1 - t_y} p_t^j$$

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<sup>23</sup>There is a long literature (e.g. Case and Shiller (1989)) that documents a small predictable component in house prices and Han (2010) and Han (2013) use price processes with some predictability. For computational reasons, we follow Flavin and Yamashita (2002); Campbell and Cocco (2003); Cocco (2005); Yao and Zhang (2005); Li and Yao (2007); Diaz and Luengo-Prado (2008) and assume shocks to house prices are permanent.

where  $r_b$  is the risk-free interest rate at which households and landlords can borrow.

Households have three potential motives for owning: the cost of renting exceeds the user-cost of owning due to the taxation of rental income, the housing ladder restricts the size of rental housing, and the several insurance motives.

## Assets

Besides housing, the only other financial asset for the household is a risk-free one period bond,  $b_{t+1}^i$ , which pays  $r_l$  to savers but costs (net)  $r_b > r_l$  to borrow. Households may borrow at this rate, subject to a borrowing constraint. Housing is the sole form of collateral. We model this by giving households a home equity line of credit.<sup>24</sup> The LTV at the time of purchase is simply the ratio:  $-\frac{b_{t+1}^i}{p_t^j h_t^i}$ .

When purchasing a home, households can borrow up to  $(1 - d)$  of the value of the house, where  $d$  is the down payment constraint. Thereafter, as long as they continue to be homeowners, agents may borrow up to  $(1 - d)$  of the value of the house. They may also choose to roll over their debt after making an interest payment. So at any time, the borrowing constraint is:

$$b_{t+1}^i \geq \min\{-(1 - d)\tau_t^i p_t^j h_t^i, (1 - 1_m)b_t^i\},$$

where  $1_m$  is an indicator variable which equals one if the household chooses to move in the period.<sup>25</sup>

If the household chooses to sell its home, it must pay off all existing debt, though another loan can be taken out if another home is purchased. A household that does not have positive total cash-in-hand (housing wealth plus financial wealth plus current income) will not be able to pay off the mortgage it has (the debt it owes) on its home and will not choose to move in this period. We do not allow the household to choose to default (see Jeske and Krueger (2005) for a model with mortgage default), but households can default implicitly by dying. After retirement, we do not allow households to take out new loans, but they may continue with their old loan<sup>26</sup>. This effectively ensures in our calibrated economy that all households reach age 75 debt-free.

Newborn households are “born” with no housing but they draw their initial wealth from a distribution  $\Pi_b$ , which is a probability distribution on  $\mathbb{R}_+$ .

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<sup>24</sup>We also call this a mortgage throughout. It is worth reiterating that there is only one asset in the model, the risk-free bond; households are not allowed to simultaneously hold “savings” and a “mortgage.” Such an alternative, if allowed, would generally be unattractive due to the higher interest rate on debt. However, because of the borrowing constraints in the model, some households might find it slightly attractive. Modeling both assets separately would require an extra state variable though.

<sup>25</sup>This borrowing constraint is different from the more typical one which restricts borrowing to be weakly less than some percentage of the house value ( $b_{t+1}^i \geq -(1 - d)\tau_t^i p_t^j h_t^i$ ). With risky house prices, for a household near the typical borrowing constraint, a fall in the value of a house means the household must reduce the amount borrowed. If house price volatility is large enough, the effective down payment constraint (the amount the household could borrow and still be able to repay in any state of the world next period) may be much tighter than the actual ( $d$ ).

<sup>26</sup>That is, a retired household’s borrowing constraint is  $b_{t+1}^i \geq \min\{0, (1 - 1_m)b_t^i\}$ .

## Household's problem

The problem of the household is to choose consumption, house size and ownership, and savings, given its permanent and transitory earnings components, housing and assets at the beginning of the period and prices, subject to budget, borrowing, and choice-set constraints and the initial condition and laws of motion for  $Q_t^j$ ,  $W_t^j$  (which we do not repeat below) for all variables<sup>27</sup>:

$$\begin{aligned}
V(a_t, L_t, \varrho_t, b_t, \tau_{t-1}h_{t-1}; Q_t^j, W_t^j, \lambda_j) &= \max_{c_t, h_t, b_{t+1}, \tau_t} [(1 - \beta)u(c_t, h_t, a_t)^{1-\phi} \\
&\quad + \beta(\mathcal{R}V(a_{t+1}, L_{t+1}, \varrho_{t+1}, b_{t+1}, \tau_t h_t; Q_{t+1}^j, W_{t+1}^j, \lambda_j))^{1-\phi}]^{\frac{1}{(1-\phi)}} \\
c_t^{\text{s.t.}} + b_{t+1} + h_t((1 - \tau_t)s_t^j + \tau_t p_t^j(\delta_j + t_p + 1 + 1_m \theta_b)) &\leq b_t(1 + r) + Y_t(1 - t_y) + h_{t-1}\tau_{t-1}p_t^j \\
b_{t+1}^i &\geq \begin{cases} \min\{-(1 - d)\tau_t^i p_t^j h_t^i, (1 - 1_m)b_t^i\} & \text{if } a_t \leq 65 \\ \min\{0, (1 - 1_m)b_t^i\} & \text{else} \end{cases} \\
r &= \begin{cases} r_l & \text{if } b_t \geq 0 \\ r_b & \text{if } b_t < 0 \end{cases} \\
c \geq 0 \quad \tau_t h_t \in \{0, H^o\} \quad (1 - \tau_t)h_t \in \{0, H^r\} \quad \tau_t &\in \{0, 1\}
\end{aligned}$$

## 5.4 Parametrization

In Section 5.5, we will compare how households that live in cities with different expected prices and volatilities behave differently in both the model and the data. Since our model is partial equilibrium and we have argued that differences in land scarcity are plausibly exogenous to differences in homeownership rates and LTV except through the cross-city variation in price behavior, the cities that we simulate with our model will differ ex ante only by their land scarcity. Parameters indexed by  $j$  vary ex ante across cities according to their land scarcity,  $\lambda_j$ . All other parameters remain constant across cities.

In this section, we discuss the calibration/estimation of some key parameters; the calibration of the remainder are discussed in Appendix B (see Table 5.1 for their values). These key parameters are all those that vary across cities and the housing ladder parameters in  $H^r$  and  $H^o$ . These are estimated in three steps.

1. We initialize the model so that the cross-section of relative prices and wages in a particular year, 2000, is the same in the model as in the data. We assume  $\sigma_\nu^j, \sigma_\epsilon^j, p_{2000}^j, W_{2000}^j, \delta^j$  vary across cities in the model with respect to land scarcity according to the same (lin-

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<sup>27</sup>Variable superscripts dropped where obvious.



ear) relationship estimated in the data.

$$\sigma_\nu^j = \alpha_\nu + \beta_\nu \lambda_j \quad (5.1)$$

$$\sigma_\epsilon^j = \alpha_\epsilon + \beta_\epsilon \lambda_j \quad (5.2)$$

$$p_{2000}^j = \alpha_p + \beta_p \lambda_j \quad (5.3)$$

$$W_{2000}^j = \alpha_w + \beta_w \lambda_j \quad (5.4)$$

$$\delta^j = (1 - \alpha_\delta - \beta_\delta \lambda_j) \delta_h \quad (5.5)$$

We run OLS regressions on equations (5.1) to (5.4), with land scarcity as the independent variable, and a range of dependent variables: local price volatility (the standard deviation over annual growth rates, 1995-2000), wage volatility, price level and wage level. We take local wage levels from the BEA data of 2000, and estimate price levels (as discussed above) from the 5 percent census extract of 2000.<sup>28</sup> The  $\delta^j$  are set using the relationship between land share and maintenance described below. This entire step can be done without computing the household's problem.

2. We simulate a set of cities with different land scarcities, each with 200,000 households. Each household is born at some date at most 54 years before 2000. For each city, we draw realizations of the annual innovations to prices and wages so that they equal their 2000 relative value in 2000.
3. We choose the parameters in the housing ladder so that specific moments in the simulated model data best match those in the data in 2000. The values of the housing ladder parameters are found by repeatedly computing the household's problem for different values of the parameters (and repeating step 2) and choosing the pair that provides the best match.<sup>29</sup>

## Housing

We assume that a city's housing supply is fixed and that homeowners pay a maintenance cost to replace depreciated housing capital. So, the (growth-adjusted) relationship between housing depreciation and housing investment is (abusing notation)

$$\delta_h = \frac{I_h - \Delta(pH)}{pH}$$

For the aggregate value of housing,  $pH$ , we use non-farm owner-occupied housing from the National Income and Product Accounts' (NIPA's) *Historical-Cost Net Stock of Residual Fixed Assets* table. Investment in housing is computed using non-farm owner-occupied housing

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<sup>28</sup>Further details about the estimates of the  $\alpha$  and  $\beta$  coefficients as well as estimates using alternative windows for measuring volatility can be found in Appendix C

<sup>29</sup>A simulated method-of-moments computed over a grid of potential parameter values.

Table 5.1: Invariant parameters

Parameter	Description	Value
$\beta$	Discount factor	0.95
$\sigma$	Housing's share in utility	0.30
$\phi$	Inverse intertemporal elasticity of substitution (IIES)	5
$\gamma$	Risk aversion	3
$t_y$	Income tax	0.20
$t_p$	Property tax	0.01
$\sigma_\epsilon$	Std. dev. of the idiosyncratic transitory shock	0.25
$\sigma_\psi$	Std. dev. of the idiosyncratic permanent shock	0.098
$r_b$	Interest rate on loans	6%
$r_l$	Interest rate on savings	4%
$d$	Down payment	0.1
$\theta_b$	Home buyer's transaction cost	0.08
$\zeta$	Replacement rate for pensions	0.6

from NIPA's *Historical-cost Investment in Residential Fixed Assets*. This gives  $\delta_h = 0.017$ . These values from NIPA are the value of the structures and do not include the value of land. For any city,  $j$ ,  $1 - \alpha_\delta - \beta_\delta \lambda_j$  is the share of structure costs in house value. So for each city, we set  $\delta_j = (1 - \alpha_\delta - \beta_\delta \lambda_j) \delta_h$ , where  $\alpha_\delta = 0.306$  and  $\beta_\delta = .470$  are the intercept and slope, respectively, of the linear relationship estimated between land share and land scarcity (see Appendix C for further details). The rent-to-price ratio in the cities will therefore vary slightly with land scarcity due to changes in  $\delta_j$ .

We allow households to choose any size rental up to a maximum:  $H^r = (0, \bar{h}^r]$ . We impose a minimum owner-occupied house size but no other restriction:  $H^o = [\underline{h}^o, \infty)$ . We use the model to set  $\underline{h}^o$  and  $\bar{h}^r$  so that 1) cities with the mean land scarcity in the model have an average homeownership rate that matches the fitted homeownership rate at the mean land scarcity in the data and 2) so that the mean ratio of owner-occupied house sizes to rental house sizes in the model matches the fitted ratio (in square feet) in the data. Matching the two moments, the homeownership rate and the relative housing sizes, identifies the two parameters uniquely. We do not have a formal proof but casual introspection (if  $\underline{h}^o$  increases, then  $\bar{h}^r$  must decrease to keep the ownership rate constant, but  $\bar{h}^r$  must increase to keep the relative house size ratio constant) and all computation thus far confirms it.

## Prices

To estimate the parameters in the price processes, we match year/city panels of house prices (from the FHFA) and average wages (from the BEA). These data are used to calculate, for each city, a covariance matrix of annual growth rates of wages and house prices over 1995-

Table 5.2: Matched parameters

Parameter	Description	Mean value	Interquartile range
$\sigma_\nu$	Std. dev of shock to wages (corr with house prices)	0.012	0.010-0.013
$\sigma_\epsilon$	Std. dev of idiosyncratic shock to house prices	0.022	0.015-0.025
$\underline{h}^o$	Min owner-occupied house size	4	4 - 4
$\bar{h}^r$	Max rental house size	8.25	8.25 - 8.25
$\delta_j$	Housing maintenance	0.0112	0.012-0.010
$p_{2000}$	Price level in 2000	1	0.81 - 1.09
$W_{2000}$	Wage level in 2000	1	0.96 - 1.02

Price and wage levels are normalized so that prices and wages are equal to one for all cities with the average level of land scarcity in the year 2000. The units on the house size parameters are median household earnings for 21 year olds, and these parameters are not changed across cities. The "interquartile range" is the difference in the predicted value of the parameter (as predicted from an OLS regression on land scarcity) between the city at the 25th and 75th percentile of the land scarcity distribution.

2000<sup>30</sup>. We have assumed in the model that  $\nu_t^j$  affects prices and wages equally. So we could use either wage growth variance or price-wage growth covariance as alternative estimates for  $\sigma_\nu^2(\lambda_j)$ . The mean (across cities) wage growth variance (0.00021) is almost twice as large as the mean price-wage growth covariance (0.00011). However, if we restrict our attention to the thirty largest cities in the sample, the two statistics do match (they are both 0.00015). We choose to use the wage growth variances to estimate  $\alpha_\nu$  and  $\beta_\nu$ .

Conditional on  $\alpha_\nu$  and  $\beta_\nu$ , house prices are used to estimate  $\alpha_\epsilon$ ,  $\beta_\epsilon$  and  $\beta_p$ . Due to the homogeneity in our model and since we only set the housing ladder parameters in a later step, we are free to normalize  $\alpha_w$  and  $\alpha_p$ . Table 5.2 shows some moments for the key parameters. The results from the instrumental variable regressions are available in Appendix C.

## 5.5 Results

### Moments in models and data

Table 5.3 shows the results from the average city by land scarcity compared to the data. Since the house size parameters were chosen so that the model matched the data on the homeownership rate and relative house sizes, it is not surprising that we attain a very good fit along these lines. The model also matches the data well if we consider only those households 65 years old and younger. Given the model's relatively simple characterization of

<sup>30</sup>Results are mostly robust to changes in sample dates with the exception that  $\beta_\epsilon$  would go *up* if the post-2007 data are used and  $\alpha_\epsilon$  would go up if the data included either the recent bust or the late 1980's bust.

Table 5.3: Model fit

	Data source	City sample	Data	Model
Homeownership rate	Census	221	0.62	0.63
Homeownership rate under 65	Census	221	0.61	0.61
Owned/rented home size ratio	AHS	42	2.07	2.05
LTV	AHS	42	0.84	0.71
LTV (conventional loans only)	AHS	42	0.80	
LTV (conventional loans only)	MIRS	25	0.77	

This table compares key parameters in the data with the model. The third column shows the number of cities on which the data estimates are based (see Appendix A for further details). The fourth column gives the mean (weighted by census sample size) for the relevant variable across those cities (NB restricting the larger samples to 25 cities has only a negligible effect on the estimated means). The AHS LTV estimates are conditional on household characteristics; see Appendix A for further details and the estimation procedure. We also report the mean LTV across cities for the sample of conventional loans in the AHS. This makes it more comparable with the LTV estimate from the MIRS (the final row), whose sample excludes non-conventional loans.

Table 5.4: Homeownership profile: data and model

Age	Data source	City sample	Data: mean	Model: mean
21-35	Census	221	0.38	0.25
36-50	Census	221	0.67	0.72
51-65	Census	221	0.76	0.88
66-75	Census	221	0.78	0.68

See notes under Table 5.3. This table reports mean ownership rates by age group.

post-retirement life, this is also not surprising. Table 5.4 shows that the model also matches the profile of homeownership relatively well, though there are too few young and too many middle-aged homeowners. We conjecture that additional heterogeneity, particularly in family size (which here does not vary within-age), would cure the excess steepness.

Though no parameters were chosen to match the LTV rates (conditional on taking a loan), the model is able to match the data from the AHS relatively well, however it is somewhat lower. This is perhaps a result of only having one non-housing asset in the model. In the data, we do not observe the mortgage net of other financial assets, which is the relevant variable in the model.

Table 5.5 shows the slopes of linear regressions of city-level homeownership and LTV on land scarcity in the data and in the model. The model is able to explain much of the difference in homeownership and LTV across cities. A ten percentage point increase in land

Table 5.5: Slopes with respect to land scarcity

age	Own: Data	Own: Model	LTV: Data (AHS)	LTV: Data (MIRS)	LTV: Model
all ages	-0.25**	-0.20 <sup>†</sup>	-0.07**	-0.08**	-0.06 <sup>†</sup>
21-35	-0.23**	-0.32	-0.09**	N/A	-0.16
36-50	-0.24**	-0.22 <sup>†</sup>	-0.05**	N/A	0.04
51-65	-0.18**	-0.09	-0.08	N/A	0.22
66-75	-0.17**	-0.13 <sup>†</sup>	0.02	N/A	0.00 <sup>†</sup>

This table compares cross-city slopes of ownership rates and LTV with respect to land scarcity, for both the data and model. For the data, reported coefficients are taken from cross-city OLS regressions (weighted by census sample size) of mean ownership or LTV (for the age group in question) on land scarcity. The local ownership and AHS LTV estimates are conditional on observed household characteristics (see Appendix A for estimation procedure), but not the MIRS. Also, there is no available disaggregation of the MIRS data by age group. Cross-city OLS regressions on simulated data weight all cities equally and do not control for household characteristics (since the sample of households in each city is the same). \*\* signifies that the estimate from the data is significant at the 95 percent confidence level. <sup>†</sup> signifies that the model estimate falls within the 95 percent confidence interval of the data.

scarcity implies a decrease in homeownership of 2.5 and 2.0 percentage points in the data and model, respectively. Likewise, the same increase in land scarcity implies a decrease in LTV of 0.7 and 0.6 percentage points in the data and model, respectively. Generally speaking, the difference in homeownership rates across land scarcity declines with age, a pattern which the model matches.<sup>31</sup>

As in the data, the relationship between LTV and land scarcity for the most part becomes less negative with age, turning positive for the older ages (although the coefficients are not significant for these ages). The shortcoming of not being able to observe net financial assets in the data is likely to be more acute for older households that have accrued savings, and perhaps explains why the increase in the coefficients is sharper in the model than in the data. Fortunately, late-life LTV figures are relatively inconsequential for the cross-city dispersion: in the data, 80 percent of new loans are taken by households under 50 and 97 percent by households under 65. Thus the restriction that, in the model, households over 65 are not allowed to take new loans is probably not important for the LTV results.

## Counterfactuals

In the data and the model, cities with high price volatility have lower homeownership rates and LTVs, but they also have high price levels, high wages, among other differences. To disentangle these different contributions, we simulate five variations to the baseline model

<sup>31</sup>This may explain why Sinai and Souleles (2005) find a positive effect on ownership from the interaction between mobility (which is negatively correlated with age) and rent volatility (which is positively correlated with land scarcity).

Table 5.6: Contribution from various elements: slopes with respect to land scarcity

Parameter	Homeownership: slopes	LTV: slopes
$\sigma_\nu$	-0.02	-0.01
$\sigma_\epsilon$	-0.03	-0.01
$\delta_j$	-0.01	0.01
$p_{2000}$	-0.23	0.02
$W_{2000}$	0.04	0.00

economy, each time allowing only one of the parameters to vary keeping the other parameters at their mean land scarcity values. Since the parameters have different implicit units, for comparability we again look at slopes with respect to land scarcity. In two of the counterfactuals, we allow the variances to vary by land scarcity according to equations 5.1 and 5.2, respectively. In a third and fourth, we simulate cities with different land scarcities so that they have relative prices or wages in the year 2000 that vary according to equations 5.3 and 5.4, respectively. In the final counterfactual, we vary the maintenance in cities according to equation 5.5. Table 5.6 shows the coefficient from regressing homeownership and LTV on land scarcity from each of the counterfactual economies, so highlighting each parameter's contribution to the cross-city differences generated by the model.

The largest contributor to the cross-city dispersion in homeownership is dispersion in the *level* of house prices. Changes in risk do affect homeownership slightly. But the results show that higher risk reduces homeownership; households, on balance, do not use homeownership to insure themselves against housing risk. Instead, the model suggests higher risk leads to lower LTVs.

### Differences in price levels and homeownership

Differences in prices create differences in homeownership through the housing ladder. In both the data and the model, households live in larger houses when they live in cheaper cities, and the difference in sizes is larger for owners than for renters<sup>32</sup>. So, households in cheaper cities, living in larger houses, are more likely to choose to own due to a binding maximum rental constraint, while households in the expensive cities are more likely to rent due to a binding minimum owner-occupied house size constraint. Likewise, differences in wages work similarly, though the total effect is smaller. Everything else equal, higher wages in the land-scarce cities leads to higher housing consumption and, due to the housing ladder, higher homeownership (consistent with findings from Coulson and Fisher, 2009).

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<sup>32</sup>The square-footage difference in the data (using the AHS, all reported differences statistically significant) between the 75th percentile and the 25th percentile city by land scarcity is 21 percent, while in the model the size difference is 19 percent of the average size house. For owners, the difference in house size is 15 percent and 17 percent in the data and model respectively. For renters the difference is 13 percent and 9 percent, respectively.

In our simulations, households respond to lower prices by increasing housing consumption, which leads to higher homeownership due to the housing ladder. Therefore, households that adjust their tenure decision in response to local prices are likely doing so to adjust their housing consumption. And so, holding housing consumption fixed, we should expect to see no effect of price on tenure decisions. In other words, prices affect tenure choices only through the price's effect on housing consumption.

In the data, a critical margin of adjustment in housing consumption is between apartments and houses.<sup>33</sup> According to our census sample, almost all houses (85 percent) are owned and almost all apartments (87 percent) are rented. Interestingly though, among owned properties, LTV ratios (predicted for 2000 from the AHS) are almost identical across dwelling types: 0.84 for houses and 0.86 for apartments.<sup>34</sup>

In Figure 5.11, we plot the relationship between conditional homeownership rates and prices across MSAs, by dwelling type: full sample, houses only and apartments only. The estimated effect of prices on ownership rates is more than three times as large for the full sample as the houses-only sample. And, the relationship for apartments is actually slightly positive. Clearly then, adjustments in housing consumption (in this case, between dwelling types) play an important part in driving the overall price-ownership relationship. Also, Figure 5.11 suggests that New York is an outlier in Figure 5.1 because it has more apartments per unit of housing than the typical city with its land scarcity.

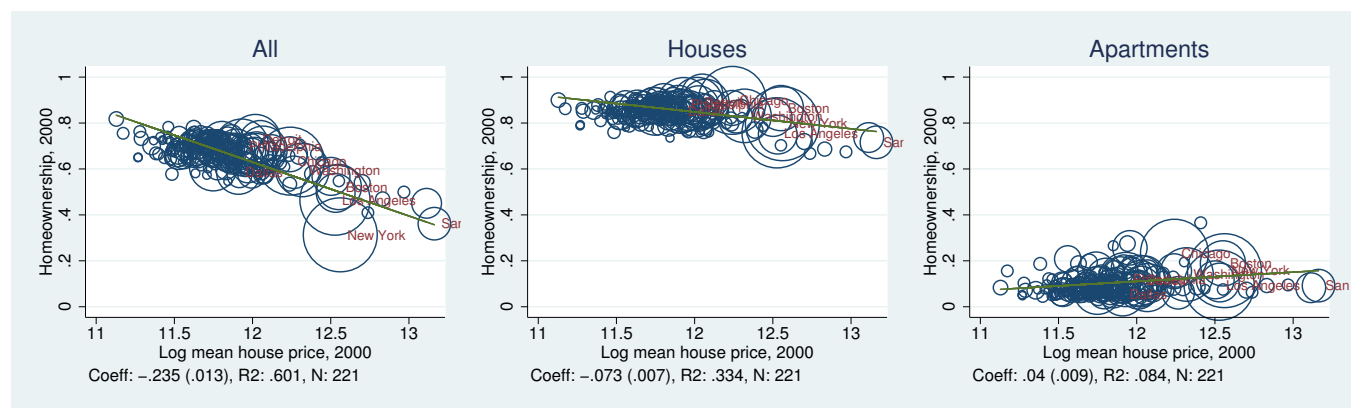


Figure 5.11: Homeownership-price relationship by dwelling type

<sup>33</sup>Based on US census data, we have defined an “apartment” as a housing unit that shares its structure with one or more other housing units; a “house” is a single-unit structure. Note that “houses” need not be entirely detached from other structures: a housing unit attached to another unit by a full-height dividing wall, that goes from ground to roof, is here defined as a “house”.

<sup>34</sup>These two statistics are means of local conditional LTVs across the 42 cities in our sample (for houses and apartments respectively), where the conditional LTVs are estimated as described in Appendix A.

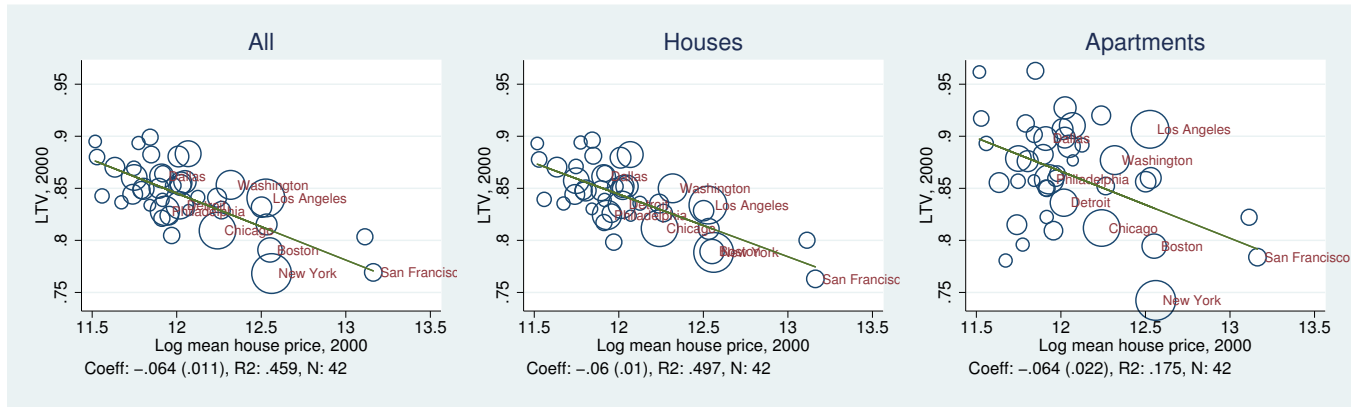


Figure 5.12: LTV-price relationship by dwelling type

In contrast to the tenure choice, the LTV decision (conditional on ownership) is not strongly related to housing consumption. This suggests the mechanism driving these LTV results is independent of the housing ladder (instead, we argue below that insurance motives are important). In Figure 5.12, we plot LTV-price relationships for the full sample, and separately for houses and apartments. This time, the effects of log prices are negative for *both* dwelling types; they are also very similar in magnitude: -0.060 for houses and -0.064 for apartments. It is clear that composition effects are not driving the relationship for LTV.

### Homeownership and within-city differences

In a general equilibrium model, the homeownership-as-insurance effect may lead to higher price-to-rent ratios (as in Nordvik (2001); Sinai and Souleles (2005); Han (2013)) rather than higher homeownership rates in cities with high price volatility<sup>35</sup>. So Sinai and Souleles (2005) looks at how differences in rental volatility across cities tilts the homeownership-by-age profile within cities. They find that riskier cities have steeper profiles, increasing faster before age 60 and then decreasing faster afterward.<sup>36</sup> This evidence is consistent with the hypothesis that households use homeownership as insurance: younger households (which are more likely to move shortly) are more likely to rent when volatility is high, whereas older households are more likely to own.

In our data and model, we too find a steeper profile in cities with high land scarcity and thus high volatility.<sup>37</sup> So we can use our counterfactuals to find the cause of the change in the

<sup>35</sup>Ortalo-Magne and Prat (2010) build a model where insurance effects imply that homeownership and price-to-rent ratios are positively correlated.

<sup>36</sup>More exactly, they impute a household's expected duration in a home using the proportion of households within the same age-occupation-education cell in an MSA that did not move the previous year. They find that propensities to own are increasing in this proxy interacted with rental volatility (see Table II, columns 2 and 3 from their paper). Halket and Vasudev (2013) show that differences in expected duration vary substantially by age - in part due to endogenous differences in tenure. Sinai and Souleles (2005) interaction result implies a steeper age profile in riskier cities (see Figure I from their paper).

<sup>37</sup>From Table 5.5, homeownership rates decline with land scarcity faster earlier in the life cycle. Our data does not show a significant steepening post age 65 however.



Table 5.7: Ownership slopes with respect to land scarcity for select counterfactuals

age	$\sigma_\nu$	$\sigma_\epsilon$	$p_{2000}$
21-35	-0.035	-0.046	-0.23
36-50	-0.026	-0.049	-0.19
51-65	-0.005	-0.010	-0.14
66-75	0.003	-0.007	-0.39

steepness of the profiles. Table 5.7 shows that the age profiles of homeownership are steeper in riskier cities and in more expensive cities. Higher risk leads to lower homeownership for all age levels. However, the change in steepness due to changes in the volatilities is consistent with the insurance hypothesis - both the  $\sigma_\nu$  and the  $\sigma_\epsilon$  slopes increase by about .03 from the age 21-35 cell to the age 51-65 cell. If, like Sinai and Souleles (2005), we used only the change in the steepness of the profiles to identify the effect of risk on homeownership, we would find that more risk leads to more homeownership. Quantitatively though, the change in steepness due to changes in price levels is almost three times as large as either change from the volatility parameters, implying that a sizable proportion of the effect that Sinai and Souleles (2005) find may not be due to risk. Instead, our model finds that most of the observed large change in age profiles is because the housing ladder is more relevant at earlier ages.

### Differences in risk and LTV

Higher risk has a small effect on homeownership, but accounts for all of land-scarce cities' lower LTVs. This is for two reasons. Firstly, households save more in the high variance cities, in part due to higher price volatility but also due to high wage volatility. Renting is a partial hedge against falls in wages that are correlated with rents and prices. However, households will not completely insure themselves against falls in wages through rental housing, since doing so would distort their housing consumption greatly. So households also hold more total wealth in the high co-variance economies. This extra total wealth leads to lower LTVs when the households do decide to purchase a house.

Model households do view homeownership as a potential source of insurance; but it is a highly imperfect variety of insurance. Housing comprises about 25 to 30 percent of expenditures in the model, so households would like homeownership to comprise about the same amount in their total wealth portfolio (including human capital) for insurance purposes. However, most renters would have to leverage their financial wealth greatly to buy a home, leaving them near the borrowing constraint and particularly exposed to large falls in house prices, which are more likely in risky cities. Large falls in prices can leave their budget sets particularly small. So households in riskier cities defer housing purchases until they can afford to buy the house with lower leverage.<sup>38</sup> This is consistent with our finding that the

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<sup>38</sup>These are two sides of similar coins: households worry about the risk that their house will be expensive

share of homeowners that purchase their house without a mortgage is not correlated with risk (or land scarcity) in the data: households with this much financial wealth in the model are not troubled by the borrowing constraint.

There are also transaction costs: more risk leads to more mobility and lower expected durations in any given house. Since adjusting owner-occupied housing is costly, this decreases the value of owning and thus the ownership rate. For instance, in the  $\sigma_\epsilon$  counterfactual, the homeownership rate for households under 35 in a typical low land scarcity city (25th percentile) economy is 2 percentage points higher than its counterpart at the 75th percentile. If we eliminate transactions costs, the (negative) slope of homeownership with respect to land scarcity halves in both risk counterfactuals. In other words, households optimally prefer to self-insure with a risk-free bond, which does not have transactions costs, does not distort the intratemporal consumption bundle and does not compel asset-poor households (which young would-be homeowners largely are) to over-leverage themselves, rather than insure with housing even though housing is the only asset whose return is correlated with the some of the risks the household faces.

Households do own slightly larger houses in the higher variance economies but there is little evidence of a housing ladder effect as discussed in Banks et al. (2010). Their theory is that households that expect to consume more housing than a rental can provide later in life anticipate owning later in life and therefore households in economies with high risk will seek to insure themselves against the risk that prices may be high in the future, when they are likely to own a large house, by purchasing (rather than renting) a small house earlier.

Theoretically, the overall strength of the “ladder effect” is particularly dependent on the nature of the housing ladder assumptions. A very rigid ladder (where, say, the minimum owner-occupied size equaled the maximum rental available, such as in Banks et al. (2010)) can potentially have large average effects early in the life cycle. However, a rigid ladder with a low maximum rental size would not enable our model to match the relative housing consumption of renting versus owning households seen in the data. More importantly, if the ladder effect were large, new homeowners should be willing to buy housing with lower down payments in order to own sooner in riskier cities. From the price level counterfactual, we do see that households would opt for higher leverage purchases in expensive cities in order to climb the housing ladder. If more risk also led households to try and climb the ladder faster via larger loans, the model would not be able to match the higher down payments (lower LTVs) in land scarce cities in the data.

Finally, the effect of differing maintenance costs is small: maintenance after all is only part of the cost of housing. Relatively high maintenance in low land scarcity economies makes owner-occupancy relatively more attractive as it increases the tax wedge in the user-cost formula, leading to very slightly higher homeownership rates and LTV ratios.

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at the time of purchase and also the risk that their house falls in value after buying it.

## 5.6 Regression-based inferences

We use a life-cycle model of homeownership to ask whether households are more likely to own when housing market volatility is higher. Our approach has been to document stylised facts based on a cross-section of cities, which we interpret using a calibrated model. Other studies on this topic have exploited variation in price or rent volatility within cities over time to empirically identify the effect on ownership. Before concluding, we wish to use the language of our model to caution against such an approach.

A household's decision to become a homeowner is a durable decision. The durability of this decision means that at the aggregate (city) level, homeownership rates are not only a function of contemporaneous prices, but also of lagged prices. For instance, take a city in the model that has high prices today but had low prices in previous years (with constant wages) and compare it to another city with the same high prices today but that also had high prices in the past. Let these two cities be otherwise identical in that households in the two cities have the same expectations with regard to price levels and volatilities going forward (i.e. these two cities have the same land scarcity value). In other words, households with the same current state values today will make the same choices in each of the two cities today. Note, however, that an econometrician using historical prices to measure current expected housing volatility<sup>39</sup> will regard the first city as having higher expected volatility relative to the second city.

In this case, the city that is measured by the econometrician as “riskier” today will also have a higher homeownership rate today relative to the second city. This is because prices in the first city were lower in the past, which led some households then to want to live in bigger houses which meant they were more likely to own (due to the housing ladder constraints). When prices rise to their current level, many households will not choose to immediately downsize, so ownership rates remain high relative to the second city. Therefore there is error in the measurement of expected price volatility which is correlated with variables that do explain differences in homeownership rates (historical price *levels*), and so estimates of the effect of expected volatility on homeownership may be biased.

Two particular attributes of this bias are worth mentioning. First, the direction of the bias will generally depend on the direction of price movements: in the above example, the bias is positive; if the first city instead had prices that fell to the same level as the second city, then the bias would be negative. Second, the magnitudes of the bias are not generally symmetric: positive shocks to prices lead to tighter borrowing constraints and so some households are unable to own, whereas negative shocks relax borrowing constraints and so some households are now able (but not required) to own.

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<sup>39</sup>Sinai and Souleles, 2005 and Banks et al., 2010 measure volatility using a 9-year window and a 5-year rolling window of realized volatilities, respectively, in their regressions. Han (2010) and Han (2013) use an AR(1)-GARCH(1,1) process to model expectations.

## Conclusion

Our model is able to explain much of the cross-city variation in homeownership and LTV and matches the variation in the data for younger households. We find that it is the relatively higher prices in cities with scarce land which causes their lower homeownership rates, while it is their relatively higher volatility that causes homeowners in these cities to borrow less. So, we do not find that more risk leads households to own more. Instead, more risk leads perhaps to a higher reliance on non-housing savings. This result highlights the importance of including other means of imperfect insurance in asset allocation models with incomplete markets. Land scarcity has a larger effect on LTV in the data than in the model. So it is probable that there are other channels through which risk may affect LTV that are precluded in our model. For instance, it is possible that households in risky cities hold the same amount of wealth but take smaller mortgages due to, perhaps, higher propensities to move and the presence of prepayment penalties that are proportional to the mortgage balance.

The main question explored in this chapter concerns homeownership and insurance. Several of the facts developed in this chapter - the relationships between land values, land scarcity and house prices, and between housing availability (the housing ladder) and homeownership - beg further examination and a full structural explanation. It would also be worth using a general equilibrium version of the model to examine the different local effects of aggregate (country-wide) shocks. Finally, it would be useful to explore whether land scarcity, via LTV, can help explain any recent geographical patterns in mortgage defaults. If our model allowed for default, households may perhaps be more likely to own in volatile cities (though this would then perhaps also imply that households would borrow more in these cities). This of course depends on the particulars of the contracting problem between lenders and households.<sup>40</sup>

## A. Data construction and supplementary estimates

### A.1 Construction of conditional ownership rates and LTVs

Here, we describe the construction of the conditional measures of local ownership rates and mean LTV used in cross-city analysis. For homeownership, we first run a probit-level regression (using the full national sample) of an ownership dummy on various characteristics of the household head<sup>41</sup>, together with a full set of MSA effects. Then, for each MSA, we use the regression estimates to predict the ownership rate corresponding to a household with the mean characteristics in each dimension.<sup>42</sup> To estimate conditional local ownership rates for a

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<sup>40</sup>See Chatterjee and Eyigungor (2011); Corbae and Quintin (2011); Garriga and Schlagenhauf (2009); Guler (2010); Jeske et al. (2011) for various models of default and home ownership.

<sup>41</sup>Quadratic in age, education dummies (high school graduate, 1-3 years of college, 4 year + of college), gender, marital status, dummies for number of children under 18 (1, 2, 3+), ethnicity dummies (black, Hispanic) and log household income.

<sup>42</sup>Because the regression is non-linear, the mean of our “conditional” ownership rates will not equal the mean unconditional rate. Therefore, we recenter all observations by a constant to correct for this.

particular demographic group (e.g. an age category), we follow exactly the same procedure, but from the beginning (i.e. even before the probit regression) restrict the sample to the relevant demographic group.

As described above, we have two alternative sources of city-specific LTV data: the AHS and MIRS. The AHS is a longitudinal survey, containing detailed information on housing-related variables. The metropolitan survey covers 41 MSAs, and booster samples of a further 6 MSAs (the largest) are included in the national survey<sup>43</sup>. Of these 47 cities, we have land scarcity data on 42; and, we base our AHS analysis on this set of 42. The AHS surveys cover different MSAs in different waves, and we therefore rely on four different waves to put together a complete sample for cross-city analysis: the metropolitan surveys of 1998, 2002 and 2004, and the national survey of 2001. We index observations by year of purchase (rather than survey year), because we have information on the loan amount and home price (to calculate LTV) at the purchase year. We restrict our sample to households with mortgages, and we only study details of mortgages which were taken out when the home was purchased. The last condition ensures that we measure the loan and price in the same year to calculate LTV.

As with the ownership rates, all reported local LTVs from the AHS are conditional on characteristics of the household head<sup>44</sup>. Also, since the AHS samples are not large, we include all households that purchased their home up to five years prior to the survey year (so the full dataset spans purchase years 1993-2004) to predict LTVs in 2000. The consequent overlapping (in terms of purchase year) of the different waves allows us to identify MSA effects. Specifically, we run an OLS regression of LTV on household characteristics, purchase year dummies and MSA dummies<sup>45</sup>. And, we predict LTV in each MSA for a household with the mean characteristics in each dimension<sup>46</sup>, who purchased their home in 2000. To

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<sup>43</sup>In the national survey, the samples for cities other than these 6 are insufficient to derive reliable city-specific statistics.

<sup>44</sup>Quadratic in age (at purchase year), education dummies (high school graduate, 1-3 years of college, 4 year + of college), gender, marital status, dummies for number of children under 18 at purchase year (1, 2, 3+), ethnicity dummies (black, Hispanic) and log household income (deflated by CPI to 2000 dollars). To predict number of children at purchase year, we count the number of children currently in the family who would have been under 18 at the purchase year; of course, children born between the purchase and survey years are not included.

<sup>45</sup>In this regression, we exclude a number of observations which have suspect LTV values. First, we exclude observations with home purchase prices and loans below \$5,000 and LTV ratios above 1.2. Second, the AHS includes a number of imputed values for mortgage size; we exclude these observations, because the imputations are not conditional on MSA. There is also a problem with top-coding, discussed in Davis and Palumbo (2008). In the metropolitan surveys, the top code values for house price and loan amount are calculated *by city* (as the mean value of the top-coded observations), which is ideal for our purposes. But, this is not the case for the (relatively expensive) cities with booster samples in the national survey: there, a *national* top code is used. In our sample, 10 percent of observations in New York are top-coded, 9 percent in Los Angeles, and 5 percent in Chicago. Our approach is to exclude all top-coded observations in the national survey from the regression. However, we take some assurance from the fact that we control in the regression for household characteristics that are correlated with top codes (e.g. household income, education); see the following footnote.

<sup>46</sup>When estimating the means of household characteristics, we include in our sample all excluded observations detailed in the previous footnote. This should partially address the problem of omitted top codes in

estimate “conditional” LTV for a particular group (e.g. an age category or loan type), we follow exactly the same procedure, but from the beginning (i.e. even before the predicting regression) restrict the sample to the relevant group.

## A.2 Conforming and non-conforming loans

Here, we consider the impact of conforming loan limits.<sup>47</sup> The existence of this nationally uniform loan limit may well be responsible for our LTV result. In more expensive cities, the conforming loan limit is more likely to bind. As a result, households will be forced to make a larger down payment (to qualify for the cheaper rates on conforming loans). And, this will yield a negative correlation between price and LTV (and consequently, between price risk and LTV too). If the conforming loan limit is driving our results, then the effect should be stronger the closer the loan size is to the conforming loan limit: increases in land value would be less likely to lead to increases in loan value if that means the household will go over the conforming loan size limit.

To test whether the loan limit is driving this effect, we check the LTV-price level/volatility correlation in samples delineated by the ratio of loan size to loan limit (restricting our attention to households with conventional mortgages). It turns out, though, that the correlation is strongly negative (especially) for loans well below the limit and less so for loans close to the limit. So, we conclude that the loan limit cannot be responsible for the correlation.

The results are reported in Table 5.8. In each case, observations are at household-level, and the dependent variable is LTV. The regressor of interest is price volatility in Panel A and log house price in Panel B. Also included are a range of household-level controls (see the table notes) and a full set of purchase year fixed effects (we only include households that purchased their home between 1993 and 2004).

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the national survey (we essentially predict the LTV of the top-coded observations, based on their observed characteristics).

<sup>47</sup>Mortgages in the United States fall into different categories. Mortgage loans may either be conventional or non-conventional. To qualify for a conventional loan, households must pass credit and income tests (known as PITI tests: principal-interest-taxes-insurance). If they cannot afford a threshold down payment (often as high as 20 percent), they will also have to purchase PMI (private mortgage insurance) to qualify for a conventional loan. Non-conventional loans are guaranteed by the government, through the Federal Housing Administration (FHA) or Department of Veteran Affairs (VA). They tend to be more appropriate for households that require a large LTV. Conventional loans may either be conforming or nonconforming. Loans are conforming if they fall below a dollar threshold, which varies with time. Until 2008, this threshold was nationally uniform (our sample excludes years after 2008). Conforming loans are subject to cheaper rates, because they are more liquid: Fannie Mae or Freddie Mac will provide guarantees enabling a lender to sell them to the secondary market. See, for example, Caplin et al. (1997) for further detail.

Table 5.8: Regressions of LTV on price volatilities and levels, for samples delineated by the loan-limit ratio

PANEL A: PRICE VOLATILITIES							
Sample: loan/limit	(1) 0-0.25	(2) 0.25-0.5	(3) 0.5-0.75	(4) 0.75-1	(5) 1-1.25	(6) 1.25-1.5	(7) >1.5
House price volatility	-4.882*** (1.653)	-1.593*** (0.586)	-0.587 (0.392)	-0.223 (0.483)	-0.552 (0.380)	-0.767 (0.455)	-0.474 (0.863)
Household controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Purchase year effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,360	4,566	3,285	1,506	550	275	304
MSAs	42	42	42	42	42	42	42
R-squared	0.051	0.020	0.007	0.008	0.055	0.099	0.169

PANEL B: PRICE LEVELS							
Sample: loan/limit	(1) 0-0.25	(2) 0.25-0.5	(3) 0.5-0.75	(4) 0.75-1	(5) 1-1.25	(6) 1.25-1.5	(7) >1.5
Log house price	-0.388*** (0.051)	-0.116*** (0.017)	-0.042*** (0.014)	-0.005 (0.021)	-0.031* (0.017)	-0.042** (0.019)	-0.055 (0.033)
Household controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Purchase year effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,360	4,566	3,285	1,506	550	275	304
MSAs	42	42	42	42	42	42	42
R-squared	0.119	0.037	0.012	0.008	0.060	0.108	0.189

Regressions are run separately for samples delineated by loan-to-limit ratio (the ratio of loan size to conforming loan limit, reported above each column). For each sample, we separately estimate the effect of price volatility (in Panel A) and log price level (in Panel B) on household-level LTV. All regressions control for the household characteristics listed earlier in Appendix A (i.e. those used to condition the local LTV estimates), as well as a full set of purchase year effects. We use the composite sample described earlier in Appendix A, though we exclude all household with non-conventional mortgages. There are 42 MSAs in the sample. SEs, clustered by city, in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

Regressions are disaggregated into samples delineated by the loan-limit ratio. Also, the sample is restricted to households with conventional mortgages (we are interested in the impact of the conforming loan limit). The loan-limit ratio for each sample is reported at the top of the columns (0-0.25, 0.25-0.5, 0.5-0.75, 0.75-1, 1-1.25, 1.25-1.5 and >1.5).

The effect on LTV is negative in all samples for both price volatility and levels. For volatility (Panel A), the effect is very large and statistically significant for the 0-0.25 sample

(-4.9) and the 0.25-0.5 sample (-1.6). But, the effects for all the other samples fall below 0.8 and are statistically insignificant. The effects around the conforming loan limit (0.75-1 and 1-1.25) actually tend to be *smaller* than elsewhere. This suggests that the negative effect is not being driven by some interaction with the conforming loan limit. The patterns are very similar for price level in Panel B, though more of the samples are statistically significant.

### A.3 Local variation in effective interest rates

An alternative hypothesis is that banks subject households in riskier cities to higher mortgage interest rates - and this could explain the lower ownership rate in these cities. Similarly, it could explain why households in these cities choose to take out smaller loans (relative to home value). Interest rates may also vary across cities because of differences in state-level regulation.

However, it turns out that effective interest rates<sup>48</sup> are actually *lower* in expensive/risky/land-scarce cities. This is illustrated by Figure 5.13, using data from the MIRS on 25 major cities. There is a strong negative correlation across cities between the interest rate and both price risk and level (R squared is 40-50 percent in each case). The interest rate varies from 7 percent in the most expensive/risky/land-scarce cities to 8 percent in the least. And so, this cannot explain why ownership rates and LTV ratios are also lower in expensive cities.

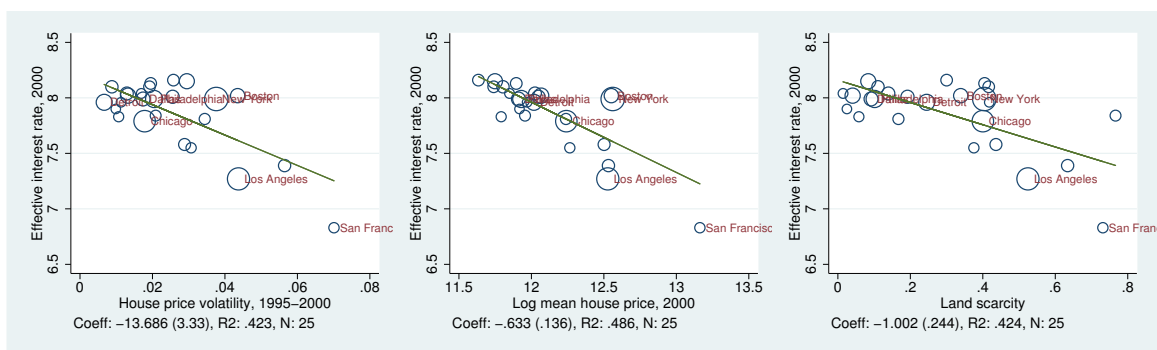


Figure 5.13: Effective mortgage interest rate, price risk and price levels

### A.4 Dynamic wealth explanations

The lower LTVs in land-scarce cities might also be explained by the contemporaneous emergence of significant local wealth, allowing homebuyers to put down large deposits (with more modest loan requirements). In particular, this wealth may originate from recent local house price growth: as argued above, land-scarce cities tend to experience larger price booms (and busts). In Figure 5.14, we plot local house price growth separately for recent decades (1980s, 1990s, 2000s) against land scarcity<sup>49</sup>. Prices grew significantly faster in land-scarce cities in

<sup>48</sup>“Effective” because it accounts for any up-front fees

<sup>49</sup>Local price growth is estimated from the IPUMS census extracts of 1980, 1990 and 2000 and the American Community Survey of 2010. The sample size in these plots varies across periods because the sample of MSAs



the 1980s and 2000s.

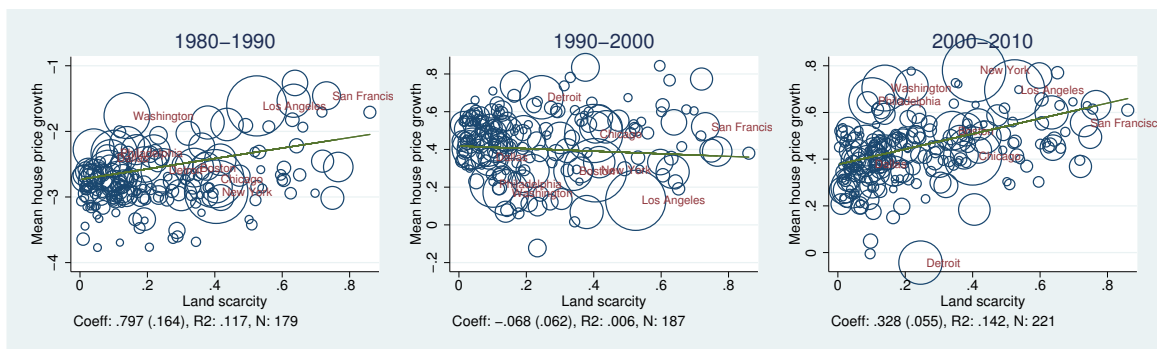


Figure 5.14: House price growth and land scarcity, by decade

If wealth effects from house price trends are responsible for the LTV results, we should find that the patterns are driven by purchases by previous homeowners rather than first-time buyers. Fortunately, the AHS data allow us to estimate conditional LTV separately for each of these buyer types. In Figure 5.15, we plot conditional LTV (in 2000) on land scarcity, separately for previous homeowners and first-time buyers. Reassuringly, the coefficient of the OLS-estimated slope is identical in each case (0.09). This suggests that housing wealth effects are not driving the results.

It is true that the down payments of first time buyers may be funded by relatives, who have benefited from growing local housing wealth. However, only a small fraction of households (5 percent in our sample) report that the main source of their down payment was an inheritance or gift. Re-estimating the results without these households makes only a negligible difference to these results.



Figure 5.15: LTV and land scarcity, by buyer type

An alternative source of expanding local wealth is contemporaneous wage growth. However, Figure 5.16 shows that the growth of average wages in the 1980s, 1990s and 2000s is not identical in each census cross-section. Note that the geographical definitions of these MSAs has also changed over time, as the cities have expanded.

uncorrelated with the land scarcity instrument.<sup>50</sup>

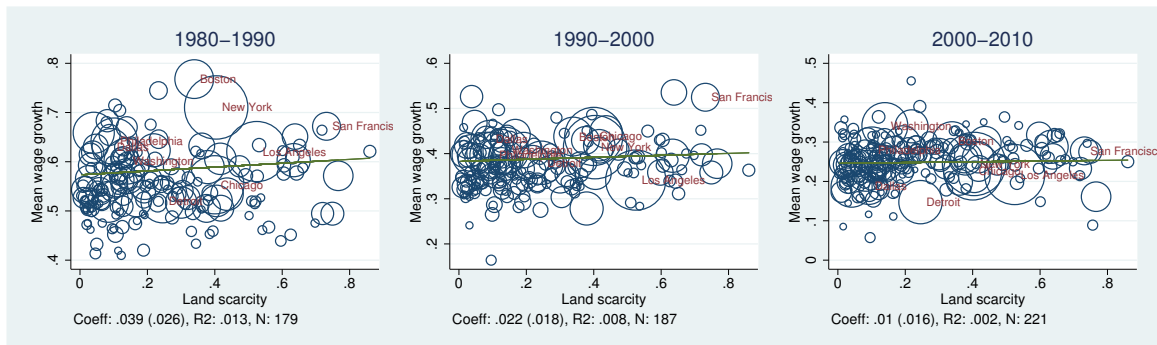


Figure 5.16: Wage growth and land scarcity, by decade

## A.5 Land share, price levels and volatilities

In this section, we explain why land share is an important factor for both house price levels and volatilities. Consider first the cross-city variation in price levels. The first panel of Figure 5.17 shows that there is substantial variation across cities in house price levels (the range covers two log points). But, comparing the final two panels of Figure 5.17, the cross-city variation in structure costs is negligible: it is land values that are driving the large variation in house prices. Clearly then, cities with larger land shares will have higher house prices. In further results in Appendix C, we confirm that the effect on price levels comes entirely through the land value component (structure costs are uncorrelated with land share).

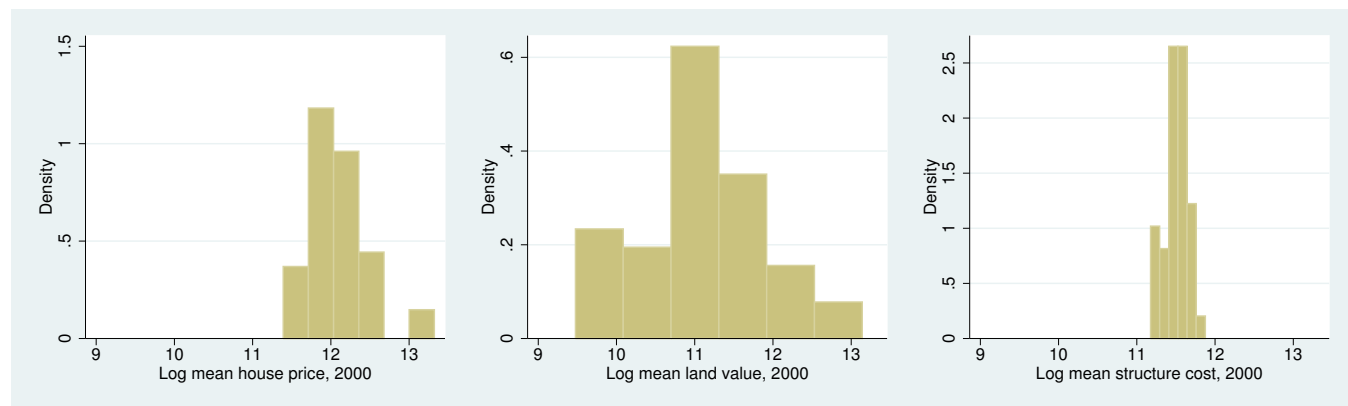


Figure 5.17: Histograms of city price, structure cost and land value levels

Next, consider the variation in house price volatilities, that is, the standard deviations over annual growth rates. As with the price levels, the first panel of Figure 5.18 reveals

<sup>50</sup>Local wage growth is estimated from the IPUMS census extracts of 1980, 1990 and 2000 and the American Community Survey of 2010.

large variation in volatilities, ranging from 0.006 to 0.075. It turns out that the covariance between the growth rates of land and structure costs within a city over time is, on average, negligible. And so, the standard deviation of house price growth over time within a city can be approximated as:

$$\sigma_j(g_{jt}^{hp}) \approx l_{j,t}\sigma_j(g_{jt}^{lv}) + (1 - l_{j,t})\sigma_j(g_{jt}^{sc}), \quad (5.6)$$

where  $l_{jt}$  is land share, and  $g_{jt}^{lv}$ ,  $g_{jt}^{sc}$  and  $g_{jt}^{hp}$  are the annual growth rates of land values, structure costs and house prices, respectively. In Appendix C, we show that the volatilities of land values and structure costs are individually uncorrelated with land shares. And, as can be seen in Figure 5.18, the volatility of land value is an order of magnitude larger than the volatility of structure costs. Therefore, according to equation 5.6, house price volatility will be strongly positively correlated with land share - through a composition effect. And indeed, Figure 5.6 confirms that both local house prices levels and volatilities are increasing in land share<sup>51</sup>.

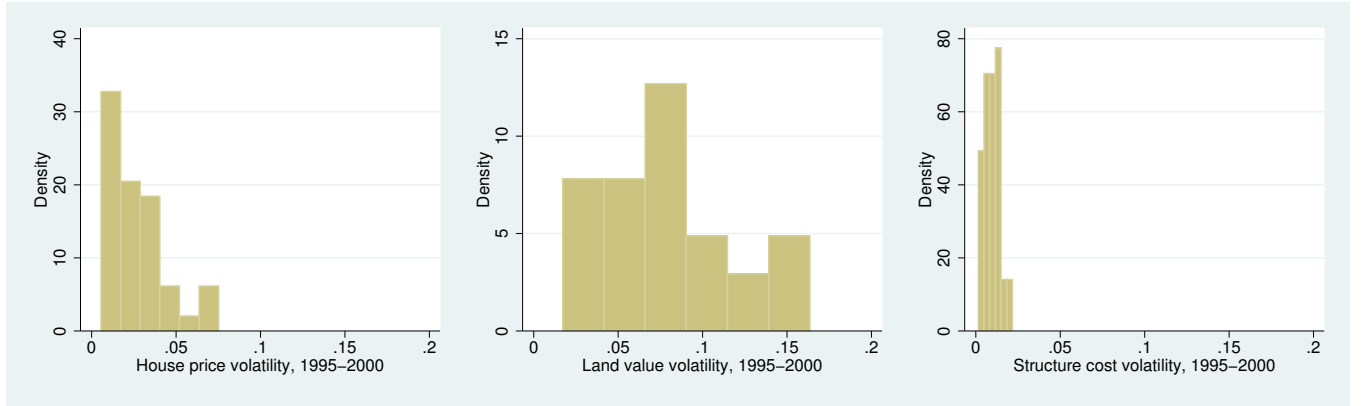


Figure 5.18: Histograms of city price, structure cost and land value volatilities

## B. Parametrization

### B.1 Household life-cycle and preferences

We calibrate the discount factor,  $\beta = 0.95$ , and housing's share in the utility function,  $\sigma = 0.3$  following Favilukis et al. (2010) and the inverse of the intertemporal elasticity of substitution,  $\phi = 5$ , following Piazzesi et al. (2007). Estimates of risk aversion vary widely, particularly when the parameter is separately identified from the intertemporal elasticity of substitution. Some studies have point estimates with  $\gamma = 20$  or higher but with equally large confidence intervals (see Attanasio and Weber (1989); Vissing-Jorgensen and Attanasio (2003), and, for values over 100, Yogo (2006)). Since such a large value of  $\gamma$  would imply an outlandish level of precautionary savings in our model, we choose  $\gamma = 3$ , which is well

<sup>51</sup>Reported land share is the mean over the four quarters of 2000, based on estimates from Davis and Palumbo (2008).

within the more traditional range of two to five that most studies prefer (see Lustig and Van Nieuwerburgh (2010); Hryshko et al. (2010); Li and Yao (2007)).

### B.1.1 Family size equivalence

We collect data from the period 1970-1993 in the Current Population Survey (CPS). We control for year effects by using year dummies. The family size profile is generated by the regression:

$$F_{iat} = \sum_{k=21}^{81} \beta_k 1_k + \sum_{t'=1970}^{1993} \beta_{t'} 1_{t'} + \epsilon_{iat}$$

where  $1_k$  is a year dummy which takes on value 1 when  $a = k$ , and  $1_{t'}$  is the year dummy that takes on value 1 when  $t' = t$ .

Figure 5.19 shows the profiles of family size from the CPS. Family size increases sharply when the household is young, peaking at age 39.

In order to adjust the household's housing and consumption stream, we use a household equivalence scale. The objective of an equivalence scale is to measure the change in consumption needed to keep the welfare of the family constant as the family size varies. Note that using per capita consumption assumes that the family converts consumption expenditure into utility flow following constant returns to scale. Lazear and Michael (1980) point to the existence of family goods, economies of scale and complementarities, which are all factors that they show to be significant. We therefore use a household equivalence scale that is not constant returns to scale. Table 5.6 lists some equivalence scales. L-M stands for Lazear and Michael (1980), US Dept of Commerce refers to US Department of Commerce (1991) and F-V&K stands for Fernandez-Villaverde and Krueger (2007). Lazear and Michael's scale takes greater account of common or public goods, so that the impact of family size is less than other equivalence scales (compare, for instance, Orshansky (1965)). We use the housing equivalence scale used by Fernandez-Villaverde and Krueger (2007).

All households in the model economy have the same life-cycle profile of family size, which is set to the average family size at each age in the CPS. To account for non-integer family sizes, we assume that the adjustment factor is linear within the family sizes specified in Table 5.6. Figure 5.19 shows the equivalent, normalized family size over the life cycle.

Table 5.9: Family size equivalence scale

Family Size	L-M	Orshansky (1965)	US Dept of Commerce	F-V&K
1	100	100	100	100
2	106	126	128	134
3	128	151	157	165
4	147	189	201	197
5	169	223	237	227

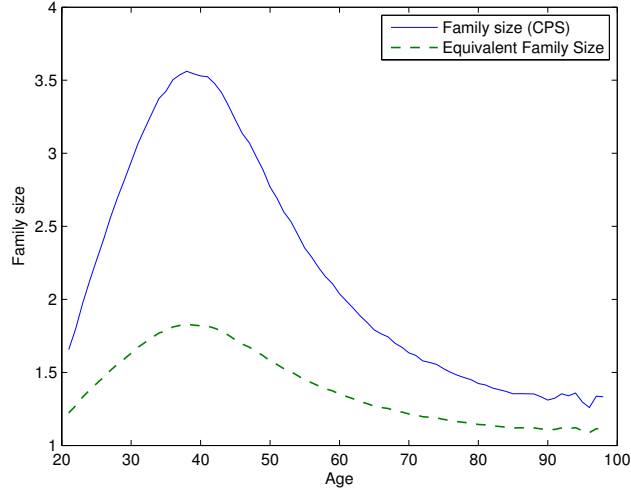


Figure 5.19: L-M adjusted family size profile (yearly bins)

## B.2 Assets

We set the down payment requirement,  $d = .1$ . We set the transaction cost of buying,  $\theta_b = .08$ , within the range typically chosen by the literature (Martin (2003); Fisher and Gervais (2011)). We set the interest rates  $r_l = .04$ . The average difference between a 30 year fixed rate mortgage and the 30 year U.S. Treasury bond is between 1 percent and 2 percent for 1977-2010 so we set  $r_b = .06$ .

### B.2.1 Initial wealth distribution

We calibrate the wealth distribution of newborns using the distribution of wealth among 21-25 year olds in the Survey of Consumer Finances (SCF) waves from 1989-2001. We drop top-coded observations, households with negative wealth, and students from the sample and use the sample weights provided by the SCF. We parametrize the initial wealth distribution as an exponential distribution. That gives us one parameter that we have to match.

$$f(b_0) = \lambda_w e^{-\lambda_w b_0}$$

where  $b_0$  is the initial wealth, and  $\lambda_w$  is the parameter to estimate in the exponential distribution. We estimate  $\lambda_w$  by matching the mean of the initial wealth distribution.

$$\lambda_w = \frac{1}{\bar{b}_0}$$

This gives us  $\lambda_w = 0.00589$ . We convert the initial wealth distribution in the data to model terms by scaling by the ratio of average labor earnings at age 21 in the model to average labor earnings at age 21 in the data.

### B.3 Taxes

There are two forms of taxes in the model economy - income tax,  $t_y$ , and property tax,  $t_p$ . Piketty and Saez (2007) use public use micro-files of tax return data from the Internal Revenue Service, which have the advantage of being aggregated to the household level already. The income tax rate we choose,  $t_y = 0.2$ , is in the same range that they compute for the US economy<sup>52</sup>.

We use data from the IPUMS 1990 5 percent sample. The variables used are the amount of property tax paid and the estimated value of the house. We remove top-coded variables from the sample, and consider only owner-occupiers. Sample observations are weighted using the household weights given in the data set. The weighted average of the ratio of the amount of property tax paid to the estimated value of the house is 0.012. In the model we set  $t_p = 0.01$ .

### B.4 Earnings process

We parametrize the idiosyncratic and age-profile portion of the household's earnings following Halket and Vasudev (2013), who estimate a process similar to Storesletten et al. (2004a) but also control for regional variability (in their case, at the U.S. state level) in earnings rather than just national variability. We set the standard deviation of idiosyncratic innovations,  $\sigma_\psi = .098$  and let the initial (fixed effect) distribution have a standard deviation of 0.5 (since the persistent component of earning follows a random walk, a fixed effect is equivalent to households entering at age 21 with a value  $\psi_{21}^i$  drawn from normal distribution with standard deviation 0.5). As is well known, the variance of the transitory shock is not separately easily identified from the variance of measurement error in these approaches to estimation. We set  $\sigma_\epsilon = .25$ , which is within bounds found by Storesletten et al. (2004b); Blundell et al. (2008). We discretize the innovations with a 3-point distribution following Tauchen (1986).

We set the pension at 60 percent of final earnings,  $\zeta = 0.6$ .

## C. Further robustness exercises (online appendix of the published paper)

### C.1 Robustness checks for homeownership and LTV effects

Here, we check for robustness in the impact of price levels and volatilities on ownership (Table 5.10) and LTV (Table 5.11). We report results for both OLS and IV (with land scarcity as the instrument), for volatilities measured using different windows (5 years, as in the main text, and 10 years also), for different year cross-sections (2000, as in the main text, and 1990 also), and for different datasets for LTV (AHS and MIRS).

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<sup>52</sup>See Table 1, page 6 in their paper

Table 5.10: Effects on homeownership of price, volatility and land scarcity

Year sample	2000			1990		
	OLS	IV	Observations	OLS	IV	Obs
Log house price	-0.235*** (0.013)	-0.254*** (0.024)	221	-0.214*** (0.012)	-0.233*** (0.022)	191
Volatility (5yr window)	-4.952*** (0.402)	-6.755*** (0.795)	221	-2.815*** (0.228)	-8.162*** (2.132)	153
Volatility (10yr window)	-4.378*** (0.323)	-5.689*** (0.642)	215	-2.630*** (0.519)	-13.390** (5.552)	98
Land scarcity	-0.245*** (0.032)	- -	221	-0.263*** (0.036)	- -	191

This table reports coefficients from linear cross-city regressions of the local homeownership rate for both 2000 and 1990 on a range of variables: log house price, two measures of volatility (5yr and 10yr windows) and land scarcity. For the IV results, we use land scarcity as an instrument for prices and volatility. Note that 1990 has fewer observations because the set of MSAs in the census changed between 1990 and 2000. Local homeownership rates are conditional on household characteristics, and are constructed as described in Section 2.1 in the main text, using the IPUMS 5 percent census extracts of 1990 and 2000. We include volatility measures (constructed as described in Section 2.1) for both a 5 year window (i.e 1995-2000 for 2000; 1985-1990 for 1990) and a 10 year window (1990-2000 for 2000; 1980-1990 for 1990). The 1990 samples are smaller because the set of MSAs has changed between years. And, the samples for some volatility windows are smaller because the FHFA time series for prices are longer for some cities than others. All regressions are weighted by census sample counts. SEs in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

Table 5.11: Effects on LTV of price, volatility and land scarcity

Dataset, year	AHS, 2000			MIRS, 2000			MIRS, 1990		
	OLS	IV	Observations	OLS	IV	Obs	OLS	IV	Obs
Log house price	-0.064*** (0.011)	-0.076*** (0.020)	42	-0.055*** (0.009)	-0.071*** (0.014)	25	-0.014 (0.012)	-0.021 (0.018)	25
Volatility (5yr window)	-0.995*** (0.329)	-1.830*** (0.612)	42	-1.153*** (0.242)	-1.695*** (0.406)	25	-0.241 (0.156)	-0.645 (0.629)	25
Volatility (10yr window)	-0.703*** (0.262)	-1.543*** (0.547)	42	-0.783*** (0.215)	-1.579*** (0.485)	25	-0.347 (0.268)	-0.908 (0.864)	25
Land scarcity	-0.072*** (0.023)	-	42	-0.078*** (0.019)	-	25	-0.028 (0.026)	-	25

This table reports coefficients from linear cross-city regressions of local mean LTV ratio on a range of variables: log house price, two measures of volatility (5yr and 10yr windows) and land scarcity. For the IV results, we use land scarcity as an instrument for prices and volatility. The first set of columns corresponds to the AHS in 2000; here, LTV ratios are conditional on household characteristics, and are constructed as described in Section 2.1 in the main text. For the MIRS, we report estimates for both 2000 and 1990. We include volatility measures (constructed as described in Section 2.1) for both a 5 year window (i.e 1995-2000 for 2000; 1985-1990 for 1990) and a 10 year window (1990-2000 for 2000). The FHFA data does not extend back sufficiently to calculate 10yr volatilities for 1990. All regressions are weighted by census sample counts. SEs in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

For homeownership, the observed patterns are robust to choice of cross-section, volatility window and year. In each case, we see the strong negative relationships described in the main text. Looking at the OLS specification, the effect of price is very similar across years: a doubling of house prices is associated with a fall in the ownership rate of between 21 and 24 percentage points. The IV results are very similar. With regard to volatility, changing the window of measurement has little effect on the coefficients for each cross-section. But, the results do vary across years: the OLS effects are larger in 2000, and the IV effects larger in 1990. Still, the reduced form effect of the land scarcity instrument (in the final row) is very similar across years.

In Table 5.11, the estimated effects are remarkably similar in magnitude across the AHS and MIRS datasets for the 2000 cross-section, for all variables. In each case, there is a strongly significant negative effect, consistent with the main text. The results are not very sensitive to the chosen volatility window either. However, the effects on LTV in the 1990 cross-section (as measured by MIRS), while negative, are all statistically insignificant. This is a result of smaller coefficients, rather than larger standard errors. In Figure 5.20, we explore this further: we plot the estimated coefficients from reduced form regressions of LTV (from the MIRS data) on land scarcity, separately by year (over 1978-2008). The dashed lines are 95 percent confidence intervals. The effect has always been negative, though it was small and insignificant until the mid-1990s (averaging about -0.5). It has grown steadily since though, reaching almost -2 in 2008.



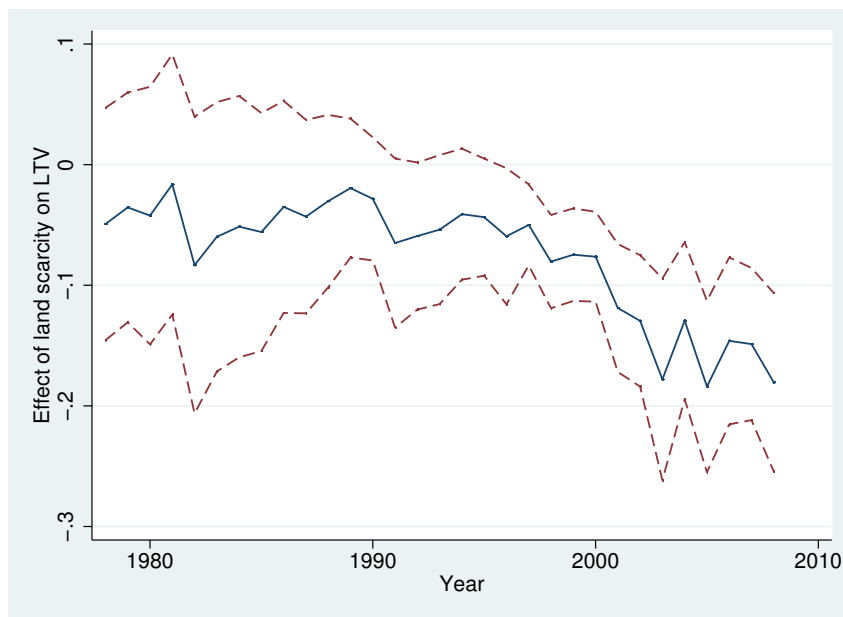


Figure 5.20: Changing effect of land scarcity on LTV

The blue line gives estimated coefficients from OLS regressions of LTV (from the MIRS data) on land scarcity, separately by year (over 1978-2008). The dashed lines are 95 percent confidence intervals.

## C.2 Disaggregation of price and volatility effects

Table 5.12 provides the detail for a discussion in Appendix A in the main text. The idea is to show that the strong positive relationship between local land share and price levels/volatilities is entirely a composition effect: the land value component (as opposed to structure cost) is larger and more volatile. The same is true when we instrument land share with land scarcity: the effect appears to be causal.

In Panel A, we regress price levels and volatilities - and their individual components - on land share, for the 2000 cross-section. This is based on the 42 MSA sample, for which we have the Davis-Palumbo land share data; in this sample, land share varies from 0.15 to 0.85. A 0.1 increase (a 10 percentage point increase) in the land share is associated with a 21 percent increase in local house prices. This effect is entirely due to variation in land value, rather than structure cost. Also, a 0.1 increase in the land share is associated with a 0.0081 increase in price volatility. The effect of land share on the volatilities of land value and structure costs are statistically insignificant. Evidently then, the positive relationship between overall price risk and land share is entirely due to a composition effect (land values are much more volatile than structure costs).

Panels B and C give the reduced form and 2SLS effects of land share on the disaggregated price levels and volatilities, where land scarcity is the instrument for land share. The IV effects in Panel C are qualitatively and quantitatively similar to the OLS effects in Panel A.

Table 5.12: Explaining cross-city variation in local price levels and volatilities

PANEL A: OLS (2000)						
	(1) Log HP	(2) Log LV	(3) Log SC	(4) Vol HP	(5) Vol LV	(6) Vol SC
Land share	2.110*** (0.151)	4.594*** (0.168)	0.128 (0.155)	0.081*** (0.012)	-0.044 (0.035)	0.004 (0.004)
Constant	11.239*** (0.072)	9.224*** (0.080)	11.505*** (0.074)	-0.006 (0.006)	0.100*** (0.017)	0.008*** (0.002)
Observations	42	42	42	42	42	42
R-squared	0.830	0.949	0.017	0.519	0.037	0.022

PANEL B: Reduced Form (2000)						
	(1) Log HP	(2) Log LV	(3) Log SC	(4) Vol HP	(5) Vol LV	(6) Vol SC
Land scarcity	0.975*** (0.241)	2.076*** (0.482)	-0.008 (0.122)	0.052*** (0.011)	0.016 (0.028)	0.004 (0.003)
Constant	11.888*** (0.088)	10.653*** (0.175)	11.565*** (0.044)	0.014*** (0.004)	0.075*** (0.010)	0.009*** (0.001)
Observations	42	42	42	42	42	42
R-squared	0.290	0.317	0.000	0.350	0.008	0.033

PANEL C: IV (2000)						
	(1) Log HP	(2) Log LV	(3) Log SC	(4) Vol HP	(5) Vol LV	(6) Vol SC
Land share	2.075*** (0.246)	4.419*** (0.276)	-0.018 (0.255)	0.110*** (0.021)	0.034 (0.060)	0.008 (0.007)
Constant	11.254*** (0.112)	9.302*** (0.126)	11.571*** (0.117)	-0.019** (0.010)	0.065** (0.028)	0.006** (0.003)
Observations	42	42	42	42	42	42
R-squared	0.829	0.948	0.000	0.449	0.000	0.000

HP is house price, LV is land value, SC is structure cost. Price levels (Log \*\*) are means over the four quarters of 2000. Volatility ("Vol") is standard deviation over annual growth rates in prices (measured at first quarter of each year), between 1995 and 2000. Instrument in IV columns is land scarcity. Observations are weighted by city size. SEs in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

### C.3 Land scarcity slopes for parametrization

In Section 4 of the main text, we described the parametrization of the model. Our method is to compare cities with different scarcity of land, which we take as an exogenous variable. These cities differ in a number of dimensions that are important for the model: specifically, levels and volatilities of local house prices and wages, and local land share. In Table 5.13, we report the OLS reduced form estimates of these variables on land scarcity, our instrument. In the main text, we use these estimates to characterize cities with high and low land scarcity; see Section 4 in the chapter for further details.

Table 5.13: Land scarcity slopes for key parameters

Dependent variable	(1) Log house price	(2) Log wage	(3) House price volatility	(4) Wage volatility	(5) Land share
Land scarcity	0.966*** (0.100)	0.193*** (0.062)	0.036*** (0.004)	0.012*** (0.003)	0.470*** (0.099)
Constant	11.760*** (0.034)	10.431*** (0.021)	0.015*** (0.001)	0.008*** (0.001)	0.306*** (0.036)
Observations	221	221	221	221	42
R-squared	0.299	0.042	0.276	0.087	0.361

Log house price is estimated for 2000 using data from the 5 percent census extract. Log wage is taken from metropolitan-level data of 2000 from the BEA. House price volatility is the standard deviation over annual growth rates in prices (measured at first quarter of each year), between 1995 and 2000, taken from the FHFA. Wage volatility is constructed in the same way using data from annual BEA data. Land share is taken from Davis and Palumbo (2008). In each case, the regressor is land scarcity (taken from Saiz, 2010). Observations are weighted by census sample size. SEs in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

As a robustness exercise, Tables 5.14 and 5.15 report the land scarcity slopes for house price volatility and wage volatility respectively, varying the time window used to calculate volatility in each column. In each table, the first column (volatility window 1995-2000) gives the estimates used in the parametrization in the chapter. The mean house price volatility grows significantly as the window is extended: the constant in the regression is more than double for the 1985-2009 window as compared to 1995-2000. The land scarcity slope also grows with the volatility window, largely due to recent cyclicalities: the slope approximately doubles when the boom and bust of the 2000s is included. For reference, if we changed our calibration sample to 1985-2000, the mean city by land scarcity would have as much house price volatility as the 75th percentile city does in the 1995-2000 calibration. We already know from the counterfactual section in the main text that that would have only a small effect on the model output, particularly with regard to homeownership.

The coefficients on wage volatility are more robust to changes in the volatility window. The coefficient on land scarcity hardly changes at all across the columns of Table 5.15. But,

the constant does grow somewhat as the window is extended: it is almost twice as large for the 1985-2009 window, as compared to 1995-2000.

Table 5.14: Robustness of land scarcity slope for house price volatility

	(1)	(2)	(3)	(4)	(5)
Volatility window	1995-2000	1990-2000	1985-2000	1985-2009	1990-2009
Land scarcity	0.036*** (0.004)	0.043*** (0.005)	0.049*** (0.009)	0.084*** (0.010)	0.094*** (0.009)
Constant	0.015*** (0.001)	0.019*** (0.002)	0.030*** (0.003)	0.033*** (0.003)	0.027*** (0.003)
Observations	221	215	163	163	215
R-squared	0.276	0.269	0.156	0.308	0.322

This table estimates cross-city OLS regressions of local house price volatility on land scarcity, where volatility is calculated using a different time window in each column. House price volatility is the standard deviation over annual growth rates in prices (measured at first quarter of each year), over the reported time interval, taken from the FHFA. The results in the main text use the 1995-2000 interval in the first column, and this result matches column 3 of Table 4 above. The sample size is smaller for intervals including earlier years, because the FHFA metropolitan sample has grown over time. Observations are weighted by census sample size. SEs in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

Table 5.15: Robustness of land scarcity slope for wage volatility

Volatility window	(1) 1995-2000	(2) 1990-2000	(3) 1985-2000	(4) 1985-2009	(5) 1990-2009
Land scarcity	0.012*** (0.003)	0.011*** (0.002)	0.009*** (0.002)	0.010*** (0.002)	0.012*** (0.002)
Constant	0.008*** (0.001)	0.012*** (0.001)	0.012*** (0.001)	0.014*** (0.001)	0.014*** (0.001)
Observations	221	221	221	221	221
R-squared	0.087	0.122	0.104	0.092	0.092

This table estimates cross-city OLS regressions of local wage volatility on land scarcity, where volatility is calculated using a different time window in each column. Wage volatility is the standard deviation over annual growth rates in prices, over the reported time interval, taken from the BEA. The results in the main text use the 1995-2000 interval in the first column, and this result matches column 4 of Table 4 above. Observations are weighted by census sample size. SEs in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

# Chapter 6

## Conclusions

The aim of this thesis is to bring new insights to old questions about urban labour and housing markets. Here, I briefly summarise the key contributions from each chapter and extensions which are worth pursuing.

The focus of Chapter 2 is the impact of immigration on native wages. The traditional approach to analysing this question is by studying the effects on marginal products of different labour types in a competitive framework. But, I show how, in a frictional model, immigration can affect native wages even if marginal products do not change: specifically, firms can exploit the relatively low reservation wages of migrants by cutting wages for all workers. I have presented indirect evidence for this phenomenon from cross-city variation in wages, employment and skill composition.

My strategy in this chapter was to demonstrate why alternative explanations for the reported empirical patterns are inconsistent with the evidence. Most importantly, I show that the displacement of low skilled natives from popular migrant destination cities was sufficient to ensure there was no significant local deviation in skill composition (contrary to the findings of Card, 2001; 2009a); this suggests the observed changes in skill wage differentials for natives cannot be explained by an influx of low skilled migrant labour driving down the marginal product of low skilled natives. In the future, it would also be useful to test whether the *magnitude* of the estimated effects on wages and employment rates are consistent with a reservation wage explanation; this is likely to require further development of the model.

In Chapter 3, I study the question of why higher skilled workers tend to migrate more between cities and states. One view is that they face relatively low costs to moving, whether because of weaker credit constraints, information constraints or psychic costs. I document new empirical evidence on this subject from direct responses to the CPS on reasons for moving. I show that better educated workers, and workers in higher skilled jobs within education groups, are more likely to move in any given year for the sake of a specific job match. But, they are *less* likely to move for reasons driven by the characteristics of the locations themselves - whether related to family, housing, amenities or job availability. Mechanically, higher skilled workers move more because the skill gradient is steeper in “match-specific” than “non-match” migration.

Using a multi-city matching model with directed search, I show how both these empirical

patterns can be explained by higher skilled workers facing larger average productivity and larger variance in job match productivity. Because of the larger potential gains from a job match, more jobs are created and both workers and firms spend more resources on search; and this explains why higher skilled workers make more match-specific moves. Geographically better integrated markets also facilitate swifter migratory responses to local declines in labour demand. The negative skill gradient in non-match moves can be explained by the resilience of large skilled job surpluses to external shocks.

As in Chapter 2, my strategy was to present evidence against alternative interpretations of the data; in particular, simple theories on skill differentiated migration costs do not sit comfortably with the evidence on non-match moves. However, it would also be useful to test whether differences in the variance of match productivity is sufficient to explain the *magnitude* of the skill gap in job and non-match migration rates; again, this would necessitate further work on the model.

Moving to Chapter 4, I document a range of new facts on local joblessness in the UK and US. In the UK since the 1980s, I show that more productive cities (as proxied by the average wage) have seen significantly higher rates of unemployment and inactivity, with the important exception of London (which is characterised by high wages and high joblessness). In contrast, in the US, there is no persistent relationship between wages and joblessness; unlike the UK, higher wages are instead strongly manifested in city size. This suggests that US cities adjust much more quickly to local demand shocks.

I cast doubt on the popular theory that Americans are somehow more intrinsically foot-loose than the British. I instead speculate that the patterns are driven by the UK's relatively generous out-of-work welfare support. This allows workers to survive long periods of joblessness in less productive cities, but it cannot explain the persistence of these shocks for so many decades. I suggest the missing ingredient is housing market adjustment: it turns out that local housing costs are so low in these cities that real wages are actually higher than in more productive areas. Historically, this may have materialised through the destruction of industry-specific human capital, while housing capital was largely preserved - causing housing costs to fall relative to wages. I also discuss the predicament of London, and suggests its distinctiveness may be driven by prolonged shocks to labour *supply*, perhaps due to persistently high rates of immigration or foreign investment in local property. Of course, all these hypotheses are speculative, and the next step for this work is to construct a model to test their internal and external consistency.

In Chapter 5, co-authored with Jonathan Halket, we challenge the popular theory that homeownership is an effective form of insurance (under incomplete markets) against rent or house price fluctuations (e.g. Sinai and Souleles, 2005; Ortalo-Magne and Rady, 2006; Banks et al., 2010). We make the novel argument that for most households, ownership is too blunt an instrument, and the accumulation of liquid savings is often a preferable means of insurance. One key contribution is to document some interesting cross-city correlations in the US. We show that, conditional on observable characteristics including income, households in cities facing higher house price risk are less likely to own and tend to opt for lower LTVs at purchase. But, this variation is difficult to interpret because of the strong positive correlation

between price levels and volatilities. To disentangle the effect of levels and volatilities, we use a calibrated life-cycle model to simulate ownership and LTV in cities with different price and earnings processes. We allow households to insure themselves through homeownership *and* through buying a risk-free bond. Our model suggests the lower ownership rates in riskier cities are a response to the high cost of housing (assuming a housing ladder where larger houses are unavailable for rent); but the low LTVs are driven by the risk itself: since households accumulate more liquid savings in riskier cities, they put down relatively larger deposits when they do eventually purchase. In this chapter, we use a partial equilibrium model, taking price and earnings processes as given; an important task for future work should be to provide structural explanations for the origin of these processes, as well as our assumptions on the housing ladder.



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